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THE NATURE OF OPTOKINETIC REACTIONS IN MAMMALS AND THEIR SIGNIFICANCE IN THE EXPERIMENTAL ANALYSIS OF THE NEURAL MECHANISMS OF VISUAL FUNCTIONS

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I. Introduction

Inquiry into the nature of the forced reactions elicited in mammals by moving visual stimuli may be shown to be of basic importance to many aspects of the psychology and physiology of vision. These responses, which are similar to the tropistic reactions of invertebrate animals (5), probably represent the most fundamental type of visual orientation which may be made by different mammals. On the other hand, because they constitute spatial orientations to local differences in the pattern of the visual field, the responses are of significance methodologically in the investigation of the mechanisms of visual functions associated with pattern stimulation, as for example, visual acuity, form vision, and real and apparent movement vision.

The forced reactions elicited by moving visual patterns in different animals are of 2 general types. In infra-mammalian forms, particularly, these adjustments consist of sustained orientation or deviation of the eyes, head, or body as a whole, but in mammals they involve principally nystagmic movements of the head and eyes in relation to the moving patterns. In general, both of these types of visual reaction have been characterized by the term "optokinetic"

or "optomotor" response. Bárány (**1**) proposed the term "railroad" *(Eisenbahn)* nystagmus in order to distinguish between nystagmus of optic origin and that elicited by vestibular stimulation. The reactions as they exist in mammals have been otherwise identified as a true tropism (**76**), as a kind of "menotaxis" (**5**), as "psycho-optic" reflexes (**67**), and as "psychic reflexes of exploratory movements" (**14**).

It is the purpose of this paper to discuss the known facts of the stimulus and neural determination of optokinetic reactions in mammals, with special reference to the problem of the neurological basis of different visual capacities. Experimental investigations of these reactions will be considered in relation to methods of elicitation, methods of recording, the behavioral characteristics of the reactions, the stimulus conditions which may be used in their elicitation, the ontogenetic and phylogenetic differences in their occurrence, and the nature of their neural control.

II. Methods of Study

A. *Methods Used in the Elicitation of Optokinetic Reactions in Mammals.* The techniques used by various investigators in evoking optokinetic reactions are much the same in principle. That is, a pattern of stimulation, consisting of either complex forms or simple stripes, is made to move across the visual field of the subject. Of the different types of apparatus used with human subjects, those most widely employed have been rotating cylinders covered with black and white lines, which are fixated by the subject either from a distance outside the cylinder (**2, 71**) or from a point within it (**18, 22, 26, 32, 82**). Flat surfaces covered with alternate black and white lines have also been used in order to induce the responses of the eyes in human subjects (**8**).

Observation of forced optokinetic reactions in both human and infra-human mammals requires that the visual field be of a thoroughly uniform nature, except for the moving patterns used in stimulation. Extraneous stationary cues permit the animal to establish fixation and consequently to hold the head and eyes still. With this fact in mind, Ter Braak (**9**), Brecher (**11**), and Smith (**84, 86**) have employed large cylinders which provide moving patterns that cover almost all of the visual field. However, if the background of the moving stimuli be of a uniform nature, it is unnecessary that the field of vision be covered with the moving patterns, since a single

stimulus will elicit the responses when it is presented against an otherwise homogeneous surround (**9**).

For some time it was believed that optokinetic nystagmus could be elicited only in the vertical, horizontal, or diagonal directions, and could not be produced when the stimulating patterns moved in a rotary direction (**26**, **49**). Noji (**67**), however, succeeded in obtaining parallel rolling of the eyes during optical stimulation.

The subject was required to try to keep a monocularly observed image of a single rotating line parallel to a stationary series of lines observed with the other eye. The maximal degree of rotation of the eyes which was observed was about 7 degrees, a value equivalent, in a given subject, to a vestibularly induced rolling caused by a sixty-degree inclination of the head. In a somewhat similar study, Brecher (**10**) required the subject to fixate the center of a slowly rotating "flicker wheel." The optokinetic rotation of one eye led to the same rolling effect of the other eye. By means of 2 discs, arranged somewhat in the manner of a haploscope, Brecher managed to induce considerable parallel as well as counter rolling of the eyes.

B. *Methods of Recording Optokinetic Reactions.* Technical problems of recording optokinetic responses arise mainly in connection with the observation of visually induced nystagmus in mammals and are not ordinarily encountered in the investigation of animals below the mammals. In the study of the tropistic orientation of invertebrates, direct methods of observing the responses of the animal have been utilized. Similarly, the quantitative measurement of the responses of lower vertebrates has been accomplished without the aid of special recording methods, although Mowrer (**62**), in his studies of the pigeon, has developed a special method for the recording of head-nystagmus. By connecting the beak of the animal with a recording lever on a kymograph, the characteristics of the responses made by the animal were reproduced.

The investigation of optokinetic reactions in mammals has been confined largely to the investigation of eye-movements which are produced by moving visual stimuli. Hence the technical problems of observation in the study of these animals deal mainly with the difficulties which arise in the recording of eye-movements under the special conditions required for the elicitation of optic nystagmus. The different methods of studying eye-movements of this sort may be grouped under 4 classes: (1) direct observation, (2) mechanical recording, (3) photographic recording, and (4) electrical recording.

The use of direct observation as a means for the analysis of eye-movements dates back to Javal, in 1878 (cited by Scofield, **82**). The observational method can still be used to advantage in many aspects

of the investigation of optokinetic reactions, especially in circumstances where special recording apparatus will interfere with the responses of the animal. Thus, such procedure as this has been found to be useful in the investigation of the ontogenetic development of optically induced nystagmus in young organisms (**105**).

Mechanical methods of recording optic nystagmus have been employed with some success in the study of different mammals.

The original mechanical method of recording eye-movements, as devised by Delabarre (**17**), has been employed by Wiedersheim (**107**) with some modifications. Delabarre's technique consisted of a metal or plaster eye-cup, containing an artificial pupil, which was connected to a writing lever by means of a string. Wiedersheim modified this method by recording the movements of the eye-cup photographically. In his extensive studies of optic nystagmus, Ohm (**69**) has made use of a mechanical lever system for reproducing movements of the eyes. In his technique, a recording arm is activated by means of a small lever which is attached to the lid or rests against the cornea itself. A modification of Ohm's methods has been utilized by Ter Braak (**9**) in the study of optic nystagmus in dogs.

Three different photographic methods of recording optically induced nystagmus have been employed.

The first of these to be mentioned is that of motion picture recording, which has been applied to the ontogenetic investigation of the reactions in children (**56**) and to the analysis of the nature of the reactions in cats lacking the visual centers of the cortex (**84**). Another photographic method of recording, one which has been developed particularly in the field of eye-movement analysis in reading (**23, 96, 99**), consists of reflecting a beam of light from the corneal surface or from a thin reflecting surface on the cornea. This technique, which is essentially the principle of the modern eye-movement camera, has been used by Ter Braak (**9**) in recording optically induced nystagmus in infra-human mammals. A third method of recording eye-movements photographically has been developed by Dodge (**21**) and has been used widely in the investigation of optokinetic nystagmus in the human individual. The apparatus in this case consists of a small concave mirror, mounted on a small block of wood, which rests lightly against the closed lid of one eye, directly over the apex of the cornea. The eccentric surface of the cornea causes the mirror to be displaced with each eye-movement in a direction opposite to that of the excursion of the eye, and with an angular displacement proportional to that of the eye. A beam of light directed toward the mirror is recorded on a moving film. The method demands monocular stimulation, since one eye must be closed in order to record the eye-movements.

Arguments have been advanced as to the relative merits of the various mechanical and photographic techniques of recording eye-movements (**43, 82**). In regard to the quantitative analysis of optic nystagmus, it may be said that disadvantages and sources of error

exist in both the mechanical and photographic methods of recording eye-movements, *i.e.* both interfere in some way with the responses of the subject or introduce a source of visual stimulation in addition to that used for testing purposes. These 2 disadvantages of observation, which can be very serious if the study of eye-movements is to be made under controlled conditions of stimulation, are eliminated by the electrical method of recording ocular movements.

The development of the electrical method of studying eye-movements may be traced to the original observations of Schott (**81**), and the later observations of Jacobson (**42**) and Meyers (**57**). The 2 latter investigators believed that the potential difference which could be recorded in a string galvanometer upon movements of the eye-ball was the result of summation of action potentials from the extrinsic eye-muscles. Mowrer, Ruch, and Miller (**65**) have shown instead that this electrical change seems to be best accounted for in terms of a corneal-retinal potential difference induced by the eye-movement. In their experiments, Mowrer, Ruch, and Miller observed a range of 0.5 to 0.75 millivolt for movements of the eyes from a median position to an extreme lateral or vertical position in different individuals, a range which is similar to that of 0.2 to 0.8 millivolt found by Fenn and Hursh (**28**) for an ocular deflection of 90 degrees. Notwithstanding such wide individual differences as these values indicate, Fenn and Hursh report that for a given individual, the magnitude of the potential change for a certain ocular deflection is relatively constant. In general, these investigators find that the potential difference developed as a result of movement of the eye-ball is a function of the sine of the angle of ocular deviation.

By providing a recording system which does not interfere with the vision of the subject, the electrical recording method is of importance in the comparative study of optic nystagmus. Smith, Kappauf, and Bojar (**87**) have utilized this technique in a comparative study of optic nystagmus in the cat and in the human individual. In this study, preliminary reports of which have already been given (**7**, **44**), the method has also been utilized in the observation of optically induced eye-movements in cats lacking the striate areas of the cortex. One of the present writers (**85**) has employed the method in the observation of the nystagmic movements of the eyes brought about by apparent rotation of the visual field.

III. Characteristics of Optokinetic Reactions in Mammals

A. *Responses Occurring During Stimulation.* Most of the available data upon optokinetic reactions in mammals deal with the characteristics of eye-nystagmus of visual origin. These responses, which

involve conjugate adjustment of eyes to moving stimuli (**29**), consist of an alternation of slow or pursuit and fast or saccadic phases of movement (**20**). The slow or tense phase is in the direction of the movement of the stimulus; the compensatory fast phase, in a direction opposite to that of the moving stimulus.

Lebensohn's (**49**) data show that the duration of the fast phase is about 0.1 second, and that of the slow component of the reaction from 1.5 to 3.0 seconds. He observed the latent period of optically induced nystagmus to be 0.2 to 0.5 second, a value which corresponds to that of 0.2 second as found by Travis (**100**) for the mean latency of the fast phase. Travis has also calculated the velocity of the moving eye in terms of the angular velocity for excursions of the visual field amounting to 5, 10, and 15 degrees. The average velocity of the eye during the compensatory rapid movements was 178 degrees per second for the five-degree excursion, 285 degrees per second for the ten-degree excursion, and 394 degrees per second for the fifteen-degree excursion. This increase in speed with the different magnitudes of movement is interpreted in terms of a gradual overcoming of the inertia of the eyeball as the extent of the movement increases. Once the eyeball is in motion, a high velocity is attained in the longer excursions.

Attempts have been made to describe the characteristics of what has been considered "perfectly adaptive optic nystagmus"(**32**). In such descriptions as these, emphasis has been laid upon the limited initial latency, the smoothness of pursuit, the correlation between the velocity of the pursuit movement with that of the moving stimulus, the close relation between the duration of the slow phase and the "interest" in the fixated stimulus, the abruptness of the onset of the fast phase, the tendencies of the fast reaction to return the eye to a position near the original point of fixation, and the occurrence of short saccadic movements of fixation within the pursuit movements in relation to surrounding objects of "interest."

In a similar vein, ideas have been advanced concerning the psychological motivation of the slow and fast phases of the movements. Bárány (**2, 3**) believed that optic nystagmus consisted of pursuit movements of the eyes in the fixation of objects seen and refixation movements designed to return the eye to an original position. Kestenbaum (**46**) and Dodge and Fox (**24**) have stated this view in more modern fashion, *i.e.* they contend that the motive for the pursuit phase is an optical impulse to hold the image of the object motionless on a given retinal area of relatively clear vision, while that of the rapid phase arises from an attempt to transfer the retinal image of a new object of relatively greater interest to an area of clearer vision. This view has been amplified by Fox and Couch (**31**) in the expression of the belief that the reactions are based upon 4

sets of factors, namely, optical factors, which serve to keep the object at the fovea; volitional factors, which serve to bring a new object of interest to the fovea; vestibular factors, which act to control orientation in space; and kinaesthetic factors, which have the effect of bringing the eyes back to their primary central position. Variations in one or another of these factors produce variations in the optokinetic adjustments. Besides external stimulating conditions, Ohm (**72**) also emphasizes " personal " factors, such as " attention," in the determination of nystagmus, a concept which has been interpreted by Bartels (**5**) in terms of the rôle of accessory sources of stimulation and their influence upon " attentional " variation in optokinetic reactions. In studies of infrahuman mammals, Ter Braak (**9**) has distinguished between optic nystagmus produced by single objects, which he believes to be volitional in nature, and that induced by a patterned series of lines, which does not demand attention or volition.

B. *After-Nystagmus of Visual Origin.* In one of his studies on the effects of rotation of the visual field, Ohm (**68**) observed that optic nystagmus of the eyes in human subjects persisted after cessation of the stimulus. The theory offered by him to explain this phenomenon is that certain pendular vibrations are set up in the visual system as a result of the intermittent stimulation produced by black and white patterns crossing the retina. Leiri (**50**) believed that such responses as these explained visual illusions of movement, but Cords (**15**) has stated that after-images of movement are of purely retinal origin and do not correspond to the nystagmus.

The methods used in the study of optokinetic reactions in mammals below the human individual have ordinarily not permitted the observation of after-nystagmus, and it is sometimes questioned whether or not these responses occur at all. But Ter Braak (**9**) mentions that the responses occurred in his animals, although he does not state in what types of animals. Smith, Kappauf, and Bojar (**87**) have demonstrated that after-nystagmus may be observed in normal cats as well as in cats lacking the visual centers of the cortex, while Smith (**85**) observed the reactions to apparent rotation of the visual field in normal cats.

IV. Conditions of Elicitation of Optokinetic Reactions

A foremost psychological problem concerning the optokinetic reactions in mammals is that of the determination of the nature of their stimulus control, for it is only in terms of stimulus correlates that the occurrence of the responses can be defined and predicted in

the intact animal. Furthermore, the understanding of this general problem is basic to a scientific description of the rôle of what has been called " interest," " personal factors," " attention," and " volition" in the determination of the responses. It is the belief of the authors that such terms may be used to designate determining factors only insofar as they are defined behaviorally and correlated with specific stimulating conditions. In this respect, it is to be hoped that the investigation of the stimulus control of optokinetic orientations may aid greatly in the scientific description of " attention," " interest," etc.

The presence and character of optokinetic responses are dependent upon a number of primary stimulus variables, particularly the velocity of the moving patterns, their width, the particular arrangement of the patterns, the type of movement, etc. In this section, an attempt will be made to describe the occurrence of the reactions in relation to these different variables.

A. *Speed of Movement.* Ehlers (**26**) has shown that the frequency of eye-movements in the human individual is a function of the velocity of the moving patterns producing optic nystagmus. The curve of the frequency of the responses rises with an increase in the speed of movement and slopes off at the higher velocities of movement. According to Roelofs and Van der Bend (**77**), the velocity of movement at which the fusion of the moving patterns occurs is dependent to a slight degree upon the particular width of the patterns employed. These authors also found that the optimal conditions for producing optic nystagmus in the human individual are found with patterns presenting 3 to 12 images per second, when the factors of speed and of width of patterns are both taken into consideration.

As shown in the experiments of Dodge, Travis, and Fox (**25**), the speed of movement also determines other characteristics of optic nystagmus besides the frequency of the responses.

They found that at angular velocities below 40 degrees per second pursuit movements can be obtained in which fixation consistently falls on one of the moving objects in the visual field. Above velocities of 90 degrees per second, several features of irregularity are noticeable, as for example, shortening, slowing, elision, and final complete failure of the responses. These values may be compared to those obtained by Smith, Kappauf, and Bojar (**87**) working with electrical recording of optic nystagmus in human subjects. In this experiment, human individuals displayed nystagmus movements, consisting of 6 or more consistent responses without interruption, at speeds varying from 86 degrees to 150 degrees per second, while irregular movements not fulfilling this criterion, but nevertheless showing the presence of visual pursuit, occurred at speeds somewhat higher than 150 degrees per second.

Data concerning the relation between optokinetic reactions and the velocity of visual movement in infrahuman mammals are very inadequate.

Ter Braak (**9**) has reported that the maximal speeds of rotation for producing optic nystagmus in the rabbit are below 20 to 40 degrees per second. The minimal speed of the responses was found to be approximately 0.4 degree per second. For the cat, maximal speeds of rotation for producing regular nystagmic movements range from 22 to 130 degrees per second (**87**). Shortening and temporary failure of the responses occur in different animals at speeds above these values, but irregular responses may be observed at velocities as high as 150 degrees per second. Sometimes at even higher velocities very slow deviations of the eyes are found.

Evidently, the maximal speed of movement which will elicit optokinetic responses is a function of 2 main factors, the fusion frequency of the eye and the maximal speed of pursuit movements of the eyes. Unfortunately, the experiments concerning this problem which have so far been carried out on mammals have involved principally the factor of velocity of pursuit rather than fusion speed. As would be expected, marked irregularity of response, induced by speeds of movement at which the motor mechanisms of the eyes are unable to operate efficiently, is a common feature of the results reported in the above studies.

B. *Kinds of Patterns.* In studies of optokinetic reactions the most commonly employed stimulating patterns are alternate black and white stripes. In the opinion of Ter Braak (**9**), the actual optokinetic stimulus in this type of pattern is not the black and white stripes as such, but rather the contours of separation between them, which act as sources of " contrast." This can be shown to be true by presenting extremely fine lines such as silk threads in the moving visual field, in which case the frequency of eye responses is approximately equal to that found with black and white stripes of a width equal to the separation of the threads (**86**). It is important to notice also that it is possible to elicit optokinetic eye-nystagmus by means of single stimuli in the field of vision (**9**).

Besides the use of lined patterns, various other types of visual forms have been employed in eliciting optokinetic responses. The reactions have been obtained in cats by moving pieces of meat in front of them, in dogs with live rabbits as the stimuli, and in the human individual with pictures and other objects as stimuli. Roelofs and Van der Bend (**77**) noted that although stripes of marked black-white contrast are more adequate than pictures in producing the

responses at slow speeds of movement, pictures seem to be more adequate at high velocities of movement. This shift may be explained by the fact that at slow speeds of movement the pictures offer a much greater amount of stimulus detail, serving to bring about irregularities in pursuit, whereas at high speeds this detail is lost and the responses are determined by simplified predominating brightness contours.

Apparently, optic nystagmus can be elicited by stimulation of any part of the retina (**73**). Dodge and Fox (**24**) have shown that the responses will occur with peripheral retinal stimulation when foveal vision is excluded by a central scotoma. Similarly, the reactions may be inhibited by visual stimuli placed either centrally or peripherally (**73**).

Patterned contours provided by lights of different wave-lengths will produce optokinetic orientations of various types. This phenomenon has been used in the study of wave-length discrimination in lower vertebrates (**38, 80**) as well as in the investigation of color-blind individuals (**70, 74**). In a red-green blind subject, Ohm (**70**) observed more marked eye-nystagmus to black and white patterns than to red and green stripes. In totally color-blind individuals he found a greater frequency of optokinetic reactions to black and white patterns than occurs in normal subjects (**74**).

Optomotor orientations may be induced in lower mammals by apparent movement of the stimulus patterns as well as by real movement of the visual field (**85**). This study thus offers a profitable means for the investigation of the physiology of the Phi phenomenon, a problem which has been of concern to psychologists for many years. The stimulating conditions necessary to produce optic nystagmus to apparent movement in such animals as the cat are obtained by illuminating a series of moving lines by means of a stroboscope. Electrical records of the eye-movements of cats taken during these conditions of stimulation show that in some animals apparent movement of the stimulus patterns is as efficient as real movement in producing optic nystagmus.

C. *Width of Patterns.* A third factor in the elicitation of optokinetic responses is the possible effect of differences in the angular size of the stimulating patterns. Ehlers (**26**) made an investigation of the relation between the maximal speeds for eye-nystagmus and the width of the stimulating lines in which stripes 0.5, 1.0, 2.0, and 4.0 cm. were used. The regularity of the responses was observed to change only slightly near the maximal velocities of movement with

these different patterns. Roelofs and Van der Bend (**77**) found a direct relation between the width of pattern and maximal velocities of movement for eye-nystagmus.

The dependence of optokinetic responses upon the width of the stimulating patterns has made it possible to utilize these reactions as indicators in the investigation of visual acuity in invertebrates (**13**, **39**, **40**), in lower vertebrates (**27**) and in mammals (**86**, **104**, **105**). In mammals the frequency of nystagmic movements is not markedly altered by modification of the width of the stimulating patterns as long as the separation between the patterns remains approximately the same (**86**).

V. The Genetic Development of Optokinetic Reactions

A. *Ontogenetic Variations.* In regard to the development of eye-nystagmus in the human infant, Kestenbaum (**45**) has reported that the first appearance of ocular movement is either simultaneous with, or a little later than, the development of the capacity to follow a moving object. This occurs around the fifth week after birth. Ohm (**71**) has expressed a view similar to this in stating that the neural mechanisms for eye-nystagmus are not present at the time of birth. According to Lebensohn (**49**), however, optic nystagmus is present directly after birth, a fact which argues against the theory that unmyelinated fibers do not function in producing the response.

The time of the first occurrence of optic nystagmus in infants has been investigated in some detail by McGinnis (**56**).

He reports that optic nystagmus occurs the first time each subject opens his eyes in an experimental situation in which a series of lines are rotating at slow or medium speeds. This was found to be true even during the first 12 hours after birth, and even in an infant who was born 1 month prematurely. Although the reactions which occur immediately after birth are similar in their essential characteristics to the responses of an adult subject, those of the infant change somewhat with increasing age. During the first 6 weeks, age exerts a more marked influence on the reactions occurring in response to slower movements of the visual field. The difference apparently lies in the gradual increase in the average number of saccadic eye-movements until a certain maximum is reached.

The development of optokinetic responses in the cat has been traced by Warkentin and Smith (**105**), using a method of direct observation of eye- and head-movements.

These investigators observed that the first responses to moving striations occur at about the fourteenth day of life, at which time the animals possess a

visual acuity not much better than 360 minutes of arc. From that time on until around the twenty-sixth day of life, the visual acuity gradually develops until the animal can respond to lines subtending a visual angle of at least 11 minutes of arc. This development in visual acuity in connection with the optokinetic reactions precedes the formation of precise localizing responses of the fore-limbs, as well as the ability of the animals to jump and make other elaborate visually controlled responses. In the cat, the first appearance of the optokinetic responses has no relation to the time after birth at which the eyes open, and hence does not seem to be dependent upon previous visual stimulation or learning. The eyes of the young kitten may open at any time between 3 to 12 days of age, but the first optic movements of the head and eyes appear regularly around the thirteenth or fourteenth day after birth.

On the basis of a study in which pigeons were blinded directly after hatching by suturing the outer eyelids, Mowrer (**64**) believes that optic nystagmus, unlike vestibular nystagmus, is probably entirely learned. When the eyes of the pigeons were opened after 5 weeks, it was observed that head-nystagmus could not be induced for some hours after the animals had had their eyes open.

B. *Phylogenetic Variations.* In both vertebrates and invertebrates various optomotor reactions involving orientation of the antennae, the limbs, or the body as a whole have been studied in relation to the nature of the visual capacities of these forms, although such observations on mammals are few in number. Arthropods and mollusks are known to make optokinetic reactions involving various bodily adjustments (**41, 78, 79**). In these forms many investigations have brought out the fact that the occurrence of the reactions is a function of intensity discrimination (**39, 108**), of visual acuity (**13, 39, 40**), of flicker fusion frequency or fusion of movement (**78, 109**), of color vision (**79**), and of pattern vision (**41, 76**). Phylogenetic differences in the presence or absence in these invertebrates of the reactions under different conditions of stimulation may be attributed largely to differences in sensory capacities.

Similarly, optokinetic orientations of the body are known to occur in many of the lower vertebrates possessing differentiated visual receptors, although lizards seem to be the lowest class in which eye-nystagmus has been observed (**52**). The presence of the reactions in fish has been shown to be related to fusion of real movement (**16**), movement vision (**34, 35, 53**), the spectral sensitivity of the eye (**38**), and color vision (**80**). In the lizards the presence of head-nystagmus has been studied in relation to visual acuity (**27**).

Eye-nystagmus to visual stimulation has been observed in all birds which have been tested, with the possible exception of the owl (**5**), but the only records which have been secured are those of head-

nystagmus. Studies of Visser and Rademaker (**101**, **102**, **103**) have been concerned primarily with the occurrence of such responses in decerebrate doves, in which the reactions can be elicited by movement of the stimuli in the horizontal, vertical, sagittal, and rotary directions. Mowrer (**61**) has studied head-nystagmus in the pigeon and has observed that after-nystagmus can be elicited in these animals when movement of the visual field has ceased.

In mammals there have been a number of investigations of eye-nystagmus, as well as infrequent observations of head and body movements brought about by optic stimulation. In addition to the extensive investigations on human subjects, data have been secured concerning the reactions in the guinea pig (**11**), rabbit (**9**, **11**, **104**), cat (**9**, **83**, **84**, **85**, **86**), dog (**9**, **12**, **37**, **47**), and monkey (**9**). Some doubt has been expressed by Bartels (**5**) that the responses could be elicited in rabbits, but recent studies have shown that they do occur.

There is little doubt that the main factors determining the variation among mammals with respect to elicitation of optokinetic reactions are differences in visual capacity, but studies so far reported permit comparisons of this sort only in regard to speed of movement. In the rabbit, according to Ter Braak (**9**), the maximal speed at which optic nystagmus may be obtained varies from 20 to 40 degrees per second. The cat, with the visual cortex either intact or removed, responds to rotating striated patterns at velocities between 22 degrees and 130 degrees per second, when consistent eye-movements are taken as a criterion of adequate response. Comparable limits for the human individual are somewhat higher than this in the same situation.

There are also important differences in the type of visual stimuli which will elicit the optokinetic reactions in various mammals. Guinea pigs and rabbits, as well as other rodents, are known to be unable to fixate and pursue single objects in the field of vision in the same manner as do higher mammals. Ter Braak (**9**) has shown by mechanical recording methods that, whereas optic nystagmus of typical characteristics may be elicited in cats and dogs by a single illuminated stimulus against a dark background, similar responses cannot be produced in rabbits. This phylogenetic difference in the effectiveness of varying stimulating conditions has its basis apparently in specific types of retinal and central differentiation, as will be brought out later.

VI. The Neural Control of Optokinetic Reactions

Some of the main questions concerning the neural control of optokinetic responses in mammals are:

1. What is the relation of these responses to the electrically excitable areas of the cortex?

2. What is the rôle of the different primary optic centers and of the cerebral cortex in the mediation of the reactions?

3. What are the facts of functional localization in the nervous system with regard to the determination of the different phases of the reactions?

4. What are the facts of functional localization in the nervous system with regard to the mediation of different visual functions associated with the optokinetic reactions?

In the present section these different questions are discussed in terms of the available neurological observations relating to optic nystagmus.

A. *The Elicitation of Nystagmic Movements of the Eyes by Electrical Stimulation of the Brain.* Observations of Michailow (**58**), which were made on young dogs, constitute the first relatively complete investigation of the excitability of the cortex in relation to the nature and development of conjugate movements of the eyes. He found that electrical stimulation of the frontal cortex during the first 24 hours after birth elicited head turning in a horizontal plane, while at the age of 3 days lateral movements of the eyes, which were often combined with vertical movements, appeared as a result of the stimulation. Conjugate eye-movements were elicited at the age of 10 days.

More recent work involving electrical stimulation methods has been reported by Levinsohn (**51**) and Bárány (**3**) on the monkey. Levinsohn noted that conjugate movements of the eyes result from stimulation of the frontal areas, from the occipital cortex, and from the angular gyrus. Of these 3 areas, the frontal areas seem to be the most excitable while the angular gyrus is the least excitable. It was also noted by Levinsohn that stimulation of one area did not eliminate the possibility of obtaining responses from other areas that were excited directly thereafter. Frontal areas were ordinarily associated with more rapid eye-movements.

According to the work of Spiegel and his associates (**89, 90, 91, 92, 93, 94**), the vestibular nuclei are important centers in the determination of lateral eye-movements, because injury of these nuclei impairs the responses as elicited by electrical stimulation of the

cortex. Vertical movements are not so impaired. Apparently, the cerebellum is not involved in either type of reaction. Spiegel and Scala (**93**) believe that the connections between the cortex and the midbrain are essential for vertical movements of the eyes. According to the work of Blohmke (**6**), lateral eye-movements of a type similar to optic nystagmus occur upon stimulation of the small-celled portion of the formatio reticularis, but they do not result from electrical stimulation of the roof nuclei of the midbrain.

In connection with the observations mentioned above, it is important to recall that Ohm (**71**) considers the vestibular nuclei to be an important stage in the integration of impulses giving rise to optic nystagmus. This view has been criticized by Bartels (**4**).

B. *The Investigation of Optokinetic Reactions in Human Clinical Cases.* Notwithstanding a number of recent papers setting forth the significance of optic nystagmus in the clinical diagnosis of lesions of the central nervous system in man (**33, 95, 97, 98**), it cannot be said that there is any consistent agreement of opinion as to the relation of the reactions to various pathological conditions.

Optic nystagmus in individuals with homonymous hemianopsia has been observed in the studies of Fox (**30**), Fox and Holmes (**33**), and Strauss (**97, 98**), in which it was observed that patients show certain deficiencies in the nystagmic movements when rotation is toward the blind side of the 2 eyes. But it is the view of Cords (**14**) that modifications of optokinetic reactions are so slight in patients of this sort as to make the responses worthless for diagnostic purposes. Controlled neurophysiological observations of German and Fox (**36**) establish the fact that defects in optic nystagmus resulting from unilateral removal of the occipital cortex are temporary in nature.

Conflicting observations have been reported concerning the effect of lesions of the angular gyrus upon optokinetic responses. Wernøe (**106**) is cited as finding no alternation in the reactions in patients with lesions in the angular gyrus, while Stenvers (**95**) finds defects in similar cases.

In accordance with the view that optic nystagmus involves "attention" or "volition" (**3, 30, 31**), assertions are found that the responses depend directly upon centers located in the frontal areas of the cortex (**33, 95**). This view is that the nystagmic movements are determined by a centripetal center located in the occipital lobes and a centrifugal center in the frontal cortex. According to Lebensohn (**49**) and Cords (**14**) the responses are completely abolished with destruction of the visuo-motor areas.

It must be said that these different theories find little support either in observations of clinical cases or in the results of studies with infra-human mammals. Indeed, available evidence on the effects of removal of cortical areas in sub-human primates (**9**) and lower mammals (**9, 83, 84, 85, 86**) seems to indicate that the cortex is not essen-

tial in the determination of the behavioral characteristics of the responses. It is possible, therefore, that in the human individual as well as in lower animals the dynamic mechanisms controlling the form of the nystagmic movements are of subcortical origin, probably closely related to the centers mediating vestibular nystagmus, *i.e.* the vestibular nuclei or the nuclei of the eye-muscles themselves.

C. *The Effects of Lesions of the Central Nervous System upon Optokinetic Reactions in Infra-human Mammals.* With reference to the nature of the neural control of optokinetic head-nystagmus in infra-mammalian forms, the studies of Diebschlag (**19**) and Visser and Rademaker (**101**, **102**, **103**) have shown that decerebration does not modify the form and character of the reactions in frogs, salamanders, and doves.

Visser and Rademaker observed after-nystagmus in their preparations, and came to the conclusion that these responses, as well as stimulus-induced responses, result from summation of optical and labyrinthine impulses in brain stem centers. Mowrer (**63**) believes that, in the pigeon, the neural control of head-nystagmus is unilateral, a condition which he assumes is associated with complete decussation of the optic tracts.

Broers and De Kleijn (**12**) and De Kleijn and Rademaker (**47**) have observed optic nystagmus in dogs after unilateral destruction of the visual cortex. They report a decrease in frequency and strength of the reactions when movement is toward the hemianoptic side of the visual field. Removal of the cerebellum had no effect upon the reactions as observed in the study of Broers and De Kleijn.

No such impairment of optic nystagmus as that just mentioned has been observed in rabbits, cats, dogs, and monkeys after complete bilateral extirpation of the striate areas (**9**, **83**, **84**, **85**, **86**). In cats lacking both striate areas, the responses display the same character as that observed before the operations, even though the animals give clear evidence of typical visual defects which result from complete destruction of the cortical centers of vision. In addition, after-nystagmus may be elicited in such cats as readily as, if not more readily than, in normal animals (**7**, **87**). Furthermore, Ter Braak (**9**) has found that lesions of the frontal lobes in dogs and monkeys fail to modify the form of the responses, while in rabbits, the reactions can still be elicited after complete decortication.

Ter Braak (**9**) states, though without presenting actual data, that maximal speeds of rotation which induce optic nystagmus in rabbits are lowered slightly by cortical lesions involving the visual areas. Similar alteration in the upper thresholds of rotation in cats with complete bilateral destruction of the striate areas has not been found (**7**, **87**). Furthermore, the variations in frequency and amplitude of the responses at different speeds of rotation typically found in normal animals may also be observed in cats without the striate areas.

These different facts, coupled with observations which will be set forth hereafter, cast doubt upon conclusions concerning the cortical

determination of the slow and fast components of optic nystagmus in mammals. Results of this general sort point to the necessity for further investigation of the rôle of the primary optic centers, the vestibular nuclei, and the nuclei of the eye-muscles and other brain stem centers in the control of the reactions.

There is no doubt, however, that cortical centers, particularly those of the occipital lobes, play an extremely important part in the determination of optic nystagmus under special conditions of stimulation, although available evidence indicates that their function does not involve direct control of the slow and fast phases of the responses. Rather, the visual areas of the cortex seem to provide a mechanism of integration by means of which various kinds of stimuli, visual and extra-visual, may act to reinforce, inhibit, or modify through learning, the stimulus control of the responses. Finally, these regions of the cortex function under special conditions of stimulation which demand visual capacities beyond the limits provided by the subcortical visual system. This question is best discussed in the section below.

D. *The Neural Centers of Optokinetic Reactions and Their Relation to the Central Mechanisms of Visual Functions.* Because optokinetic orientations constitute a type of pattern discrimination, the study of the stimulus control of these responses in animals with lesions of the nervous system provides the psychologist with an experimental approach to the neurological analysis of such functions as pattern vision, visual acuity, real and apparent movement vision, and other capacities which involve or may be related to pattern stimulation.

The fact that mammals such as cats, dogs, and monkeys (9, **86**) lose the ability to give optokinetic responses to movement of a single stimulus after removal of the striate areas of the cortex seems significant in explaining why many previous investigators have considered these animals to be blind. For example, the conclusion of Munk (**66**) and Minkowski (**59**, **60**) that extirpation of the occipital lobes in dogs and monkeys brings about complete blindness is based in part upon the fact that their animals could not fixate and pursue by eye- or head-movements certain objects held or moved in the visual field.

Ter Braak (9) has explained the loss of optokinetic reactions to single visual patterns after destruction of the occipital lobes in dogs in terms of the abolition of a type of "visual" nystagmus, which depends upon "attention." Another explanation of this impairment is in terms of an actual loss in the capacity of these operated animals to respond to definite conditions of stimulation. In the attempt to

evaluate experimentally these 2 different points of view, one of the present writers (**86**) has determined the threshold distances for the angular separation of moving black or white lines which will elicit optic nystagmus in cats lacking the visual centers of the cortex. The 2 different sets of lines, both of which were made up of stripes subtending approximately 11 minutes of arc, were rotated in each case against uniform contrasting backgrounds. It was observed that 6 operated animals would not respond to the white lines when these stripes were separated by angular distances greater than 17.2–26.4 degrees. Similar thresholds for black lines rotating against a uniform white background were 2.9–7.4 degrees. The lines were located at a distance of 50 cm. in front of the animals' eyes. These results would seem to show that the "attentional" deficit in optic nystagmus resulting from removal of the occipital lobes can be defined in terms of a specific loss in visual capacity in relation to certain kinds of stimulating conditions.

The inability of these higher mammals, after the removal of the visual cortex, to respond to single patterns or patterns widely separated in the field of vision is explained most intelligibly by assuming that focal or foveal vision is destroyed by the operation. The absence of the reactions under similar conditions in the normal rodent may be accounted for by the complete absence of a central or focal retinal area in these mammals.

The persistence of optic nystagmus to complex patterned contours after complete abolition of the cortical connections with the retina (**9**, **83**, **84**) may be taken as evidence for a type of pattern vision mediated by subcortical centers. It seems rather clear that the responses produced by movement of these complex patterns in the field of vision result from a type of summation of stimuli which is not a necessary factor in the cortically mediated reactions to patterns widely separated in the visual field.

In accordance with ideas set forth here, one of the present writers (**86**) has determined the "minimum separable" visual acuity of cats after complete bilateral removal of the striate cortex by noting the presence of optokinetic responses when alternate black and white lines of equal width were rotated about the animal.

Three animals displayed a visual acuity of at least 11 minutes of arc in this situation. Using a situation in which lines were separated by an angular distance of approximately 2.9 degrees of arc, the "minimum visible" visual acuity of six animals was found to be 0.5–0.7 minute for white lines, 4.1–8.2 minutes for black lines. These observations show, in regard to visual acuity,

that the subcortical mechanisms of the optokinetic reactions are differentiated to an extent equal to the cortical centers of vision.

Studies previously mentioned seem to point to a similar conclusion in regard to the cortical and subcortical mediation of movement vision at different velocities of rotation. It will be remembered that fusion speed for optic nystagmus in rabbits (**9**) and in cats (**87**) is not markedly altered by extirpation of the visual centers of the cortex.

The importance of this general method of studying the neural basis of different visual capacities may be illustrated further by an investigation of apparent movement vision in normal cats and in cats lacking the occipital lobes (**85**). In this study apparent movement of the visual field was produced by illuminating a rotating striated pattern by means of a stroboscope, and eye-nystagmus produced in the animals was recorded electrically. Results showed that the optic nystagmus produced by apparent rotation of the visual field is as marked in the operated animals as it is in the normal animals. In both the normal and the operated cats, the responses were consistent enough to give significant proof of differential orientations to apparent movement. These data, besides pointing to a similarity in the neural mechanisms of real and apparent movement in vision, seem to prove also that the dynamic processes producing apparent movement are in part subcortical in nature.

It seems clear that these different observations have an important bearing upon theoretical formulations as to the rôle of various centers of the nervous system in the mediation of visual functions, although the data obtained consistently fail to conform to current opinion concerning this general question. In the studies cited above, there is no indication that pattern vision is completely dependent upon centers located in the striate areas of the cortex as suggested in the studies of Lashley and Frank (**48**), Marquis (**54**, **55**), and Spence and Fulton (**88**). The view that the cortical centers of vision are the only neural centers sufficiently organized to mediate a highly refined visual acuity (**75**) is refuted by the results and interpretations presented above. In regard to apparent movement vision, the results add no support to those ideas which have considered this visual capacity to be a highly refined perceptual response resulting from "physiological configurations" or dynamic "short-circuiting" in the highest centers of the nervous system. Rather, it is suggested that this visual function, which has many features in common with real movement vision, is probably controlled by the nervous system in much the same way as real movement vision, and is subject to the

same type of behavioral and neural analysis as are the responses of animals to real movement of visual patterns.

VII. Summary and Conclusions

Some of the main points which have been brought out in the present review may be summarized as follows:

1. The understanding of mammalian phylogenetic differences in the optokinetic reactions requires extensive psychological study of the stimulus control of these responses. In rodents, the reactions cannot be elicited by movement of single patterns in the field of vision but depend upon complex patterns by means of which the eye is alternately stimulated by contours of light. The maximal velocities of movement at which the reactions occur seem to be greatest for the human individual, somewhat lower for the cat, and lowest for the rabbit among different mammals which have been investigated. Thresholds of minimal velocities have been determined only for the rabbit.

2. In man, and probably in lower mammals, variation in the width of patterns eliciting optokinetic reactions does not seem to influence markedly the frequency of the responses providing the number of visual contours stimulating the retina per second is kept constant. This fact seems to show that the responses are a result of the contours of light striking the retina rather than the areas of brightness stimulating the retina.

3. The presence of after-nystagmus of optic origin has been clearly demonstrated in both birds and mammals.

4. A definite sequence in ontogenetic development may be observed in optokinetic reactions shortly after birth. Associated with their development is a gradual increase in visual acuity. Available data conflict to a certain extent concerning the significance of learning and maturation in the development of the responses.

5. Evidence is reviewed which tends to show that the vestibular nuclei may play an important rôle in the determination of optic nystagmus in the horizontal plane. Vertical movements of the eyes may not involve identical relations. The exact importance of the midbrain in the determination of these responses is yet to be ascertained.

6. Unilateral removal of the occipital lobes in the dog and in the human individual seems to have some influence on the nature of optic nystagmus, but controlled neurophysiological studies on the human individual indicate that the impairment is of a temporary nature.

7. Investigations are cited to prove that complete decortication in rabbits, or complete bilateral removal of the striate cortex in rabbits, cats, dogs, and monkeys, fails to modify the form of optic nystagmus elicited by complex patterns in the visual field. These observations tend to show that the slow and fast phases of optic nystagmus are determined subcortically.

8. The absence of optokinetic reactions to single patterns or patterns of lines too widely separated in the field of vision in higher mammals after destruction of the striate cortex is very possibly associated with a specific loss in visual capacity related to foveal vision. It seems plausible that this defect in visual capacity is a result of the elimination of the central mechanisms of the fovea or of the area centralis. The absence of optic nystagmus under similar conditions in normal rodents seems to be best explained in terms of a complete lack of an area of acute vision in these forms.

9. Because optokinetic orientations are a type of pattern discrimination, they may be utilized in the analysis of the neural mechanisms of such psychological capacities as pattern vision, visual acuity, real and apparent movement vision, or any other visual function which may involve or be related to pattern stimulation.

10. Different experiments are cited to prove that highly developed visual acuity, movement vision, and apparent movement vision are mediated through the subcortical mechanisms of the optokinetic reactions in the absence of the cortical connections with the retina.

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AN "EXPERIMENTAL" STUDY OF THE PSYCHOLOGICAL CORPORATION

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"Based on 5,000 personal interviews in 60 cities and towns in 40 states, made by 463 trained interviewers under the direction of 60 psychologists associated with the Psychological Corporation," and gathered from February 18 to March 2, 1937, a recent study [1] presents in percentages the total replies and those of 4 economic groups to 11 questions pertaining to "Sit-Down Strikes," the "Supreme Court," "Communism," "Big Business," "Advertising," "Religious Forces," and "Other Aspects of Public Relations."

The technique of "measuring public attitudes on broad social and advertising problems" is one that has grown popular in a democracy which is curious concerning its future and alert to capitalize trends for commercial purposes. The *Fortune* and Gallup polls and the market surveys of private industry are, of course, other well-known agencies employing modern statistical and sampling methods.

If it may be permitted to wave aside the point that such a measuring gadget at the moment is almost worthless as a contribution to a systematic science, no matter how useful it happens to be for practical living, it is possible to question the tone of the conclusions which the Psychological Corporation draws from its own data. Even the most casual examination of the interpretative statements in this bulletin indicates that the Psychological Corporation has an overwhelmingly strong bias in favor of "big business" and that it does not hesitate as "a part of the experimental [*sic*] program" to carry on promotion in its own behalf. As proof, quotations without italics can be presented; the question of whether the bias is good or bad is irrelevant to this discussion, as it is to any scientific analysis.

"In general, do big companies or small companies treat their workers better?" (question 6). Fifty-four per cent favor big companies, 34% the small ones, and 12% "don't know." From which distribution the following comment (quoted *in toto*) is derived:

[1] The Psychological Corporation, *A Study of Public Relations*. New York: The Psychological Corporation, March 15, 1937.

"Here again, is a result indicating great confidence in large companies as contrasted with small companies. We doubt that the large corporations are capitalizing their advantages in building up better public relations."

These results, the fact that on another question (5) 75% think that large companies do more good than harm (14% believe the reverse and 11% don't know), and "other studies of public relations" convince the Corporation that "the picture is far from being as black as usually painted." The picture referred to is that of the public reaction to large corporations.

Question 7 shows that 62% of the total people interviewed think sit-down strikes "wrong," 23% "right," and 15% "don't know." "Taken shortly after the General Motors strike, these results are amazing." Why are they amazing? "Even in the D group [the lowest economic group], more people say *wrong* than *right*"; the Corporation is apparently not amazed by the fact that there is some correlation between economic status and attitude toward sit-down strikes, or at least it does not comment on this relationship.

Another result (question 9) is also "amazing" to the Corporation, viz., that 20% "believe that the United States is on the way to Communism." In addition to an expression of amazement, the following comment is added:

"In view of the fact that probably only 1 or 2% of the total population favors communism, and even less in the A and B economic groups . . ." and "In the light of other questions and current events, we interpret it to mean this; that a large part of the public in all classes look on current events and policies as leading toward chaos, *i.e.*, communism. We intend to follow this trend with further studies."

The identification of "communism" with "chaos" may cater slightly to the tastes of the Corporation's announced list of clients (Eastman Kodak, Johns Manville, Socony-Vacuum, American Telephone and Telegraph, etc.), and it indicates a startling naïveté in regard to a well known social theory.

Bias alone, and not the data that have been supplied, produces a statement that "C and D groups are less sceptical of advertising than are the more intellectual A and B groups." Men and women were merely asked on this question (2) to rate cigarette brands in respect to sincerity and insincerity of advertising. Group differences are revealed, but there is no basis for inferring skepticism, or lack of it.

"Do you believe that the United States Supreme Court should have six new judges?" (question 10). After the data, these sentences appear:

"Note the non-controversial, non-emotional manner in which this question is put. The proper framing of questions is a critical factor in results. Our experiments show that *one word* can radically change people's response."

Not only can one word change people's response, but the major distortion in the President's plan which this question represents can produce confused or misleading results.

The "broad questions, even though general and leading in character" (number 8 on religion) "indicated that a certain idealism and searching for higher things still exists, even, yes maybe especially, because of the disturbing influences of our times." It is also "a bad omen" that the higher economic groups ("the intellectual, well-to-do people from whom leadership should be expected") are more "sceptical about religion" than the lower ones.[2]

Any man, even a psychologist or a physicist, can do what he wishes when he draws conclusions or philosophizes from his own data. Any group, including the Psychological Corporation, is certainly entitled to put its services at the disposal of business in as ingratiating a fashion as possible. And the employees of business, like Edward L. Bernays, are expected to help "stimulate" the consumer more effectively by ascertaining his "needs" or "desires." But when the Corporation announces on its letterhead that it is "a nation-wide organization of psychologists of recognized standing"; when it repeats in its "word of explanation" that it is "strictly scientific and non-partisan, has no sales force to promote its work and is non-profit making"; when it uses the names of prominent, really scientific psychologists as officers and directors; and when it tries to smuggle in its biases by creating deliberately a scientific aroma in order to promote its own prestige, it may cause a few psychologists who blush whenever they look at the popular sins committed in psychology's name to feel still more chagrined. Nor does it seem unreasonable for someone to request that this high-sounding mask be ripped aside.

[2] *Cf.* Henry C. Link, *The Return to Religion*. New York: The Macmillan Company, 1936. Pp. 181.

SHIFTY AND MATHEMATICAL COMPONENTS

A Critique of Anastasi's Monograph on the Influence of Specific Experience upon Mental Organization

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In a recent paper [1] I described the most common errors in current applications of factor analysis in psychology. Some of these errors are due to misunderstanding of the logic of factor analysis. The factorial methods are being applied ineffectively by many psychologists so that it seems imperative to show as clearly as possible just what these common errors are in the light of present knowledge. The criticism and the recommendations that I have made may be more effective if they are restated directly in terms of a published study that was ruined by inadequate methods of analysis. It is especially serious when the authors attempt to draw inferences about psychological problems from faulty experiments and erroneous factorial methods.

The purpose of this paper is to set out, as clearly as possible, and in nonmathematical form, the types of factor analysis which I consider appropriate for psychological problems and the reasons why various factor methods produce so-called "mathematical" and "statistical" factors that have little or no relation to psychological ideas.

As an example of inappropriate factor analysis I have selected the monograph by Anastasi on "The Influence of Specific Experience upon Mental Organization." [2] This study is noteworthy because of the significance of the psychological problem that the author set out to solve, and because of the meticulous care with which the experimental work and the statistical analysis seem to have been done. Perhaps the author should not be blamed for not keeping posted on the rapid development of factor analysis. She naturally tried to give a psychological interpretation of her findings, but the experimental arrangement was weak and the factor analysis faulty so that none of

[1] L. L. Thurstone, Current Misuse of the Factorial Methods. *Psychometrika*, 1937, **2**, 73–76.

[2] Anne Anastasi, The Influence of Specific Experience upon Mental Organization. *Genet. Psychol. Monog.*, 1936, **18**, No. 4, 245–356.

the conclusions of the monograph can be justified. Our interest here is in clarifying the nature of the errors involved so that another study can be made of the same problem with some hope of psychologically significant results. Our object here is to aid in solving Anastasi's problem and to aid in other psychological investigations that may be significant with proper factorial methods.

When a set of tests has been factorially analyzed, each test can be described in terms of the factors or abilities that are demanded by the test. A test of digit span, for example, can be described as demanding certain abilities but not others. The relative saturations of the factors or abilities in a test show the relative weights of the abilities in the test performance. One can also appraise each individual subject as regards the several factors or abilities. This is done in terms of his test performances and the saturations of each ability in each test. A factor analysis gives, then, 2 types of result, namely: (1) Each test is described as regards the several abilities involved in it; and (2) each individual subject is rated as to each of the abilities or factors.

The factors are usually to be interpreted as traits or abilities of the individual subjects. There is nothing in factor analysis that differentiates between those factors which are determined by the social environment and those factors which are determined by the native endowment of the subjects. That differentiation must be made by criteria outside of factor analysis as such, at least as far as it is known today. The factors or traits that are isolated may be native or environmental in origin, or both.

It is an important psychological problem to determine whether the factorial composition of a test remains constant from one occasion to the next, how it may change with different age groups, or with subjects of various types of education and social experience. For example, a routine perceptual speed test for educated adults may be an inductive reasoning test for children. It is also an important problem to determine how the abilities of individuals change in time, at what ages the several abilities mature, how the abilities may be developed by formal instruction and by experience and maturity. Still another important problem is to determine to what extent one ability can serve vicariously for other abilities. If most people normally do a task with a certain ability, it may be possible for some individuals, deficient in that ability, to do the same task in some indirect way with other abilities that they do possess. Here are 3 related types of problem, namely:

(1) The abilities demanded by a particular task may change with age, with formal instruction, with social experience, or by intentionally changed methods of doing the task. The factorial composition of a test may be changed. It is invariant only under certain conditions.

(2) The abilities may develop in the individual at different rates; some of the abilities may be determined by native endowment, some of the abilities may be subject to considerable development with instruction and with experience. The factorial description of the individual subjects, their mental profiles, may change in time.

(3) Some of the abilities may serve vicariously for other abilities in certain types of task so that people differ in the abilities that they bring to bear in doing the same task.

This problem is not as hopeless as it may seem. As regards the factorial description of a test we must demand that the factorial saturations remain invariant when the test is moved from one test battery to another with the same common factors, provided that the subjects are of the same age and education and that we do not alter the nature of the task. If a group of subjects have taken a set of tests, then the factorial composition of each test must remain invariant in all test batteries that can be taken from the larger set as long as each sub-battery contains the essential common factors. As regards the factorial ratings of individuals, we should expect that their abilities remain invariant over short periods of time, barring only exceptional and gross disturbance. We should expect their abilities to develop with age and perhaps with instruction and experience. As regards the vicarious functioning of one ability for another, we can study it experimentally by asking the subjects to tell us how they solve the test problems, and we can regroup our subjects accordingly for factorial analysis, or we can retest them after instruction in special methods of doing the tests. Experimental and analytical methods are available already for solving all of these problems.

Anastasi's problem was a section in this larger setting. She gave 5 tests to a group of children. One hour of special instruction was then given on how to solve the test problems in 3 of the tests. One should expect the factorial composition of these tests to change because many of the children shifted to the new methods of doing the tests. After one hour of special instruction on 3 of the tests, the whole battery was given a second time. This gave 2 sets of intertest correlations, namely, (A) the intertest correlations before special instruction, and (B) the intertest correlations after special instruc-

tion. The 2 occasions, A and B, were separated by 13 days. The obvious interpretation is here that the tests were altered factorially in that the same test was done with different abilities after instruction. The mental profiles of the children probably remained practically the same for 13 days.

The initial test battery contained 5 tests. But only 2 common factors can be determined in a battery of 5 tests. The reason for this limitation has been described elsewhere in relation to factor theory,[3] but it can perhaps be made clear here in nonmathematical form. The common sense of any scientific explanation demands that the explanation must be simpler than the phenomena that it unifies. We apply this criterion intuitively even when we do not write it out in mathematical form. An absurd, extreme case would be to postulate a separate instinct for every possible thing that an animal can do. There would be as many instincts as there are actions to be explained or comprehended, and consequently the explanation would be rejected. It would serve no purpose in the economy of thought. Another example would be to take 2 observations and to represent them on a graph. The 2 points can, of course, be connected by a straight line. One might conclude that there is a linear relation between x and y as represented by the straight line on the chart. But a straight line can always be drawn through 2 points. The equation for a straight line has 2 constants. Now if there are only 2 observations and if they are used to determine the 2 constants, then we have as many constants as there are observations to be explained. We reject such an interpretation as indeterminate. If there are 20 observations that are represented by as many points on the graph, and if they all fall along a straight line, then we have more observations than constants, 20 to 2. Then we are justified in concluding that the relation is a straight line, and not a curve. Such a conclusion could not be drawn from only 2 observations because the relation might be a curve, and we would not know it.

Applying this simple common sense to the factor problem, we have 10 experimentally-determined correlation coefficients with a battery of 5 tests. The diagonal terms are not experimentally determined. The reliabilities cannot be used except under certain circumstances. They exclude only the variable errors but not the specific factor in each test. Since the specific factor is unknown, the diagonal entries are also unknown at the start. The solution to the factor

[3] L. L. Thurstone, *The Vectors of Mind.* Chicago: The University of Chicago Press, 1935. Chapter II, p. 77.

problem is in the form of a factorial table which shows the saturation of each factor in each of the tests. If there is only 1 common factor in a battery of 5 tests, then such a table has only 1 column and it has 5 values. Ten experimentally-determined coefficients are sufficient to determine 5 entries so that 1 common factor could be determined. If there are 2 common factors, then the factorial table has 5 rows and 2 columns, or 10 entries. The 10 experimentally-determined correlation coefficients are just barely enough to determine the 10 entries in the factorial table, but it would not leave us convinced. A scientific explanation should always be overdetermined in order to be convincing. If there are as many as 3 common factors in the battery of 5 tests, then the factorial table has 15 entries. That is more than the number of experimentally independent observations, and hence 3 common factors can certainly not be determined from a battery of 5 tests. The same kind of reasoning can be extended to any size of test battery.

Anastasi selected the 5 tests so that they would be diversified. "The tests were selected so as to include verbal, numerical, and spatial material and to call into play a variety of different processes. The attempt was likewise made to include tests which were not too highly correlated with each other at the start in the population tested" (p. 281). The whole study was lost at the very start. It did not matter what factorial methods the author used after she had started with a test battery that has no psychologically unique solution. It is unfortunate that the labor represented in the monograph was wasted on an indeterminate test battery because this error could have been avoided by using more tests.

The first and second form of each of 5 tests gives 10 tests in all. The intertest correlations for the whole set of 10 tests are shown in Table 15, page 318, of Anastasi. We have tried to factor this augmented test battery in the hope of finding a unique configuration, but unfortunately the number of factors is too large for a psychologically unique solution.

Anastasi used the principal axes solution in factoring the 2 correlation tables A and B. The resulting factorial matrices are shown in her Tables 17 and 18, page 323. I used the principal axes solution in my earlier attempts in factor analysis, and I first described this solution in 1932. In a mathematically elegant paper Hotelling[4]

[4] Harold Hotelling, Analysis of a Complex of Statistical Variables into Principal Components. *J. Educ. Psychol.*, 1933, **24**, 417–441, 498–520.

described this solution and named it the method of principal components. In the same paper he also described an ingenious iterative procedure for obtaining the components of the test vectors on the principal axes in the total factor space. Anastasi used this iterative procedure.

The principal axes solution that Anastasi has used is one that I discarded some years ago because it almost necessarily gives psychologically meaningless results like those of Anastasi. The solution is useless even if the original test battery is determinate. The principal axes solution and the attempts to interpret this solution constitute perhaps the most common error in factor analysis. I have described elsewhere the reason why I discarded the principal axes solution,[5] but I shall attempt to state the reasons here perhaps even more directly than I have done before.

Consider a battery of tests and let this battery contain a particular test such as Opposites. As psychologists we are interested to know something about human abilities, and we assume that the subjects are using their abilities in producing the test performances. Let the test battery contain a large number of verbal tests and very few numerical and spatial tests. Then the first principal axis will lie close to the verbal tests because they characterize the battery. The principal axes are determined entirely by the test battery as a whole. The Opposites test will have a high saturation in the first principal factor. It will have a high first component. This would lead us to the conclusion that it takes a large amount of this first ability to do well in the Opposites test.

Now we construct another battery and we introduce the same Opposites test into this new battery. Let us assume that this new battery contains mostly numerical tests and that there are very few verbal tests. The first principal axis will now lie near the number tests. The Opposites test will have a low saturation with the first principal factor. Its first component will be small in the new battery.

Suppose that the test of Opposites is the first one in each of the 2 test batteries. When the Opposites test is considered as a part of the first battery, we say that it demands a lot of the first component ability. When the Opposites test is considered as a part of the second battery, we say that it demands very little of the first component ability. The abilities required in the Opposites test which the subject may take on Monday will be determined by the rest of the tests that

[5] L. L. Thurstone, *The Vectors of Mind*. Chicago: University of Chicago Press, 1935. Chapter IV.

the subject may take on Tuesday! It should require no mathematical profundity to understand that such a result is absurd.

The principal components that enter into each particular test are entirely a function of the rest of the battery of tests. Each principal component is a hodge-podge of all of the mental abilities involved in the whole battery. None of the components are psychological entities. The principal components vary with the test battery depending on the nature of the tests and on the number of each kind of test in the battery. But we want to find the mental abilities that are attributes of individuals. As psychologists, we are not satisfied with shifty mathematical artifacts.

We want to know what abilities are involved in each test, independently of the rest of the tests which may or may not be included in a particular test battery. The principal components for any test can be made to take almost any positive or negative values, depending on how the test battery is assembled. In order to isolate the separate mental abilities we must have an analysis of the data by which the factorial composition of each test becomes independent of the rest of the battery. The factorial composition of a test must remain invariant as we move it from one battery to another among all the tests that have been given to the same group of subjects, provided only that each sub-battery contains the essential common factors. The principal axes solution fails in this fundamental requirement. It does not make psychological sense.

For any given test battery the principal axes solution is unique. If several investigators analyze the same correlation table by this method, they will all get exactly the same components for all of the tests to any number of decimal places. The analytical form of the principal axes solution is mathematically interesting, and in certain psychological problems some features of it can be used to advantage.[6] The trouble is that every possible test battery has its own unique solution so that any particular test can have almost any components, depending on the other tests that may be combined with it into a test battery. Students of factor theory should study the principal axes solution for its analytical interest, but they should realize that for purposes of psychological interpretation it is useless unless it is modified.

The argument has been made that the principal axes solution should be used for psychological test problems because it is possible to determine probable errors for the principal components, whereas

[6] *The Vectors of Mind*, Chapter VII, p. 173.

no probable error formulae have as yet been derived for other solutions that are psychologically meaningful. Here we have a choice between a nonsensical solution with probable errors and other solutions that make psychological sense but for which the probable errors are as yet unknown.

In turning to the conclusions of the monograph, consider again that an overt performance may be determined by: (1) the abilities of the subject, (2) the objective nature of the task, and (3) the manner in which he does the task. The abilities are attributes of the subject; the task is defined by the objective end result. A simple example will suffice. The subject does a task that can be facilitated by visual control, or by tactual control, or by auditory control. He makes one attempt. You show him how to do the task more effectively by adding or combining two sense modalities. He makes another attempt and his performance is improved, or perhaps he finds the new method less advantageous. If several tasks enter into the experiment, their intercorrelations might change markedly. But this does not prove that the mental organization has been changed all around. The visual acuity of each subject *might* have remained exactly the same on the 2 occasions. He drew on different abilities to accomplish the same objective task.

Anastasi does not believe that the abilities of her subjects remained essentially stable for 13 days. She says (pp. 332–334): "It might be argued that the same fundamental underlying factors are present in both factor patterns, but that they manifested themselves differently . . . A factor is frequently regarded as a common ability or reaction tendency which enters into the subject's performance . . . but which is itself independent of other abilities and unitary, . . . Such unit abilities . . . might conceivably remain constant, although concrete performance on specific tests altered. . . . Any assumption that an underlying factor remained constant would be entirely conjectural . . . such a factor, furthermore, could never be investigated directly by any method." The burden of proof is on the author to dislodge the simplest and the most obvious interpretation of her experiment. Her indeterminate case can neither prove nor disprove that the mental abilities of her subjects changed markedly in 13 days.

It is conceivable that basic human abilities can be altered by formal instruction or by experience. In the present state of knowledge we should regard the abilities as fairly stable characteristics of each person at least within short periods of time, such as 2 weeks. There is no necessity for assuming that the basic human abilities are

eternally fixed. Just as visual acuity changes with age, so may also the mental abilities develop and decline, and they may be subject to special tutorial and experiential effects. It is probably rare for one hour of class instruction to alter markedly any of the basic human abilities, and if Anastasi really altered the mental organizations of a whole class of children, then it surely was a rare and fateful class hour!

We turn next to the author's statement that factors as mental abilities can never be directly investigated by any method. It should be of interest that this problem has been solved and that the solution has been applied to test data with psychologically sensible results. The solution is in the discovery of what has been called a simple structure [7] in the configuration of psychological test vectors. I shall attempt here a nonmathematical description of the general nature of a simple structure or configuration. Consider the factor patterns of Tables 17 and 18 (p. 323). If the test battery had been longer, and if the factoring had been properly done, then the factorial table would show the saturation of each factor in each test. But the reference frame is ordinarily arbitrary. For every factorial table there is an infinite number of tables which reproduce equally well the experimentally determined correlation coefficients. Which one of all these factorial tables has psychological meaning?

The answer to this question is crucially important if factorial methods are to be used in experimental psychology. Let us make the trial assumption that there is some limited number of mental abilities. Let us assume that we do not know how many mental abilities are involved in the tasks that we ask our subjects to perform, and that we do not know anything about the nature of any of them. They might be such powers as ability with numbers, ability to visualize, auditory imagery, ability to memorize, verbal ability, rhyming ability, and so on. We assemble a list of, say, thirty tests, and we determine their intercorrelations for a group of subjects. The factoring is done so that the number of columns of the factorial table will be as small as possible, subject to the condition that the factorial table must represent the correlation coefficients.

If we knew what the abilities are and how they function in each test, then we could write the factorial table, and we should then expect to find a large number of zero entries in the table. This follows from psychological considerations and not from mathematics. The Opposites test, for example, probably does not involve number

[7] *The Vectors of Mind*, Chapter VI.

ability. A speed test in addition probably does not call for rhyming ability, or memorizing, or auditory imagery. Each test might demand several abilities, but it is not likely that there would be a single test in the battery that would demand all of the mental abilities represented in the whole list of 30 tests. It can be shown mathematically that it is always possible to rotate the reference frame so that a certain number of zero entries are introduced in the table, but we can easily determine the maximum number of zero entries that can be introduced into an arbitrarily constructed factorial table. In all of these rotations of the reference frame we are, of course, restricted by the fact that the factorial table must represent the correlation coefficients in the experiment. Within that restriction there is an infinite number of factorial tables that produce the same correlation table.

By purely psychological considerations we are led to the belief that there is one reference frame for the factorial table that will reveal a very large number of zero entries, much larger than can be obtained in a factorial table constructed by chance. We search for it, and we find such a table. It reproduces the experimental correlation coefficients, and it has a very large number of nearly vanishing entries. We know that such a table could not be found if we started with a table constructed by chance. We inspect the non-vanishing entries in the table, and we find that all large negative values have disappeared! This did not happen by chance either. We look at the tests that are high in the first column, and we might find that they are all number tests. Furthermore, all of the number tests have significant saturations in this column. We look over the tests which have nearly vanishing entries in this column, and there are no number tests in that list. We do likewise for each column, and we find that they make psychological sense. When such a situation can be found in a psychological test battery, we call the reference frame a simple configuration or structure. We have then found what we expected by purely psychological considerations, namely, a factorial table with a large number of zero entries. In other words, each test contains a few of the factors. None of the tests contains all of the factors. All of the factors that are significant for a test enter into the test positively so that there are no negative abilities in the battery. There is nothing in the mathematics of the problem that would indicate the existence of such a solution, but psychological common sense indicates that such a solution should lie in the data—and we find it. Furthermore, we take a different battery of tests, and we give it to another

group of subjects. The factorial analysis is done with the tests identified by code numbers. A simple structure is found again, and the same psychological factors appear! All of this does not happen by chance. These findings give encouragement to the belief that there are such things as mental abilities and that it is possible to identify them.

In the concluding statement of the monograph the author says: "Factors should be empirically defined in terms of the concrete relationships from which they are found, and should be regarded as 'shifting, mathematical components' rather than as fixed, underlying psychological entities." I should insist that the factors be regarded as psychological entities with some degree of stability but that they need not be regarded as rigid and fixed. I doubt whether anyone does regard the factors as immutably fixed. If factor analysis is to be of any use in psychology, then the factors must be psychological entities. They must make psychological sense. If they are only shifting mathematical artifacts, then they are useless for psychology.

When Anastasi's problem is again studied experimentally, the result would almost certainly be more conclusive with the introduction of several modifications. The test battery should be longer, perhaps 20 or 30 tests. Interpolated instruction could be given on 3 or 4 tests. The A and B forms of the instruction tests could be incorporated into the same battery, or the whole battery could be given twice with instruction on only a few of the tests. The non-instruction tests should be sufficiently numerous and so selected that they serve as adequate landmarks in the common factors of the battery. The correlation table should be factored by the centroid method [8] or by some other equivalent method in which the diagonals are regarded as unknown communalities. After factoring, the reference frame should be rotated to a simple structure if such a set of reference axes exists. There is no guarantee beforehand that any particular battery is sufficiently determinate to reveal a simple structure, especially if the test battery is short. If the non-instruction tests remain essentially in the same configuration in the A and B batteries, then one should be justified to infer that the mental abilities of the subjects probably remained essentially the same for the 2 occasions. The abilities of the subjects might be estimated independently from the 2 batteries in order to determine whether the individual abilities shifted in the time between the A and B tests. The shift in the nature of the instruction tests

[8] *The Vectors of Mind*, Chapter III.

could be interpreted in terms of the other primary factors in the battery, and they should agree with the nature of the interpolated instruction. For example, if the subjects were taught to use Euler's circles or similar graphical devices in solving the syllogisms, then we might expect the reasoning test to shift toward the primary factor of space in the second testing.

Factorial experiments should preferably be made to test psychological hypotheses. For example, an inductive factor was found in one of my experiments.[9] It was interpreted psychologically as the ability to discover the rule or principle which unifies some given material. The familiar Number Series test is one example. The psychological question is raised whether this interpretation is correct and whether this mental faculty transcends the nature of the immediate content such as spatial, verbal, and numerical material. A set of 20 tests was retained from a previous test battery so as to identify 7 primary mental abilities. Ten new tests were devised so as to represent inductive thinking on a variety of content. The new battery contained 30 tests. If our present interpretation of the inductive faculty is correct, then all of the 10 new tests will be heavily saturated with it. If this does not happen, then we have either guessed wrong about the nature of this factor or about the psychological nature of some of the tests. We might have to start over again with another psychological hypothesis. This experiment is now in progress.

The question has been raised whether the mental faculties that are found by the factorial methods should be named and given psychological interpretation. In my judgment this is the only way in which the factorial methods will serve experimental psychology. For example, one of the faculties is identified with simple numerical tests. It is less saturated in those arithmetical reasoning tests in which the subject must comprehend and formulate a problem before doing the numerical work. This factor has appeared before as a group factor, and it was found by Kelley and others in multiple factor studies. This factor has been called Number, and it has been denoted N. As yet we do not know whether this factor is to be identified as a part of the native endowment of the subjects or whether it is almost completely determined by the common school experience of the subjects. Perhaps it is determined in large part by native endowment. But there is some difficulty in conceiving

[9] *Primary Mental Abilities.* Chicago: The University of Chicago Press. In press.

this ability with numbers to be natively determined because numerical operations are essentially cultural rather than biological. Eventually we might find non-numerical tests that are heavily saturated with this factor and that would lead to a better understanding of the factor. Perhaps it is a biologically more basic faculty that happens to be well represented in numerical work. In that case it would be merely a historical accident that the faculty was discovered in numerical tasks. It is immaterial whether the name is then retained because of such a historical accident, or whether the name is changed in accordance with a better understanding of the nature of the factor. The factor might be found to be socially and educationally determined, in which case there would be no difficulty in defining the faculty as numerical.

The psychological description and interpretation of mental faculties need not imply any neurological point locus for each faculty nor even a specific organ for each of them. Some of the mental faculties may be in the nature of parameters in the dynamics of the physiological system without representation in any specific organ or point locus. We may be able to discover some of the mental faculties long before they become known in terms of physiological dynamics. Eventually the 2 approaches to this problem must agree.

The question is sometimes raised whether the mental factors are unitary in the sense of an all-or-none action. It seems extremely unlikely that any of the factors determined by the factorial methods will be of this character, but it would be exciting if such factors could be found. The mental faculties isolated by the factorial methods are probably not ultimates. They will surely break down into further elements. We are justified in talking about the arms and legs of a man as functional units even though we realize that his arms and legs can be further subdivided toward smaller and smaller units. Why not regard the mental faculties in the same manner? The fact that they show uniqueness in the test performances, appearing again and again in different groups of subjects, taking different psychological test batteries, does indicate that they represent some functional unity. I doubt whether any competent investigator in this field would seriously interpret any of the present mental factors in terms of the genes of mental inheritance.

Another question concerns the orthogonality of the factors. The factorial methods do not necessarily impose that the primary factors shall be uncorrelated. The investigator may impose orthogonality if he so prefers, but the factorial methods are flexible so that he

may allow the primary factors to be correlated if that is his preference. Even when he allows this freedom, the primary factors may turn out to be uncorrelated. I have found this situation with adults, and it gives further strength to the solution. This question of orthogonality has been a source of frequent confusion by critics who do not understand the logic of factor analysis. There seems to be some indication that the primary factors for children are more likely to be correlated than the primary factors for adult subjects. The cause may be in a maturation factor or in Spearman's "g." It must be remembered that differential rates of maturation among the primary factors are not completely eliminated by merely correcting the correlation coefficients for point age. Very interesting psychological problems are associated with the interpretation of oblique primary factors.

Factor analysis is developing very rapidly so that the best methods now available may be obsolete within a few years. I should not insist on any particular method of analysis. But any methods that we use must be consistent with psychologically acceptable postulates. The mathematical form of a factorial solution is secondary to the question whether it is adapted to the psychological problem. We cannot blame mathematics or the mathematicians for absurd results if we use analytical methods that are unsuited to our own problems. This is a plea that we demand from factor analysis psychological entities instead of "shifting mathematical components" so that factorial studies may constitute valid additions to psychological knowledge.

SPECIAL REVIEW

Newman, Freeman, and Holzinger's Twins: A Study of Heredity and Environment *

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Whenever a book or monograph appears on the subject of twins, it is currently sure to receive attention, and whenever such a book appears under the joint authorship of an eminent biologist who is also an authority on the biology of twins, a noted psychologist, and a well known statistician, one can be certain that its conclusions will receive widespread recognition. Unfortunately a study which is the result of the pooled intellectual resources of such well qualified individuals is apt to be accepted uncritically especially by the lay scientist and the educator. For this reason, and because the study is vulnerable, the present writer ventures to set forth certain more or less obvious criticisms.

Before turning to the weaknesses of the study, certain points of strength should be noted. The approach of the authors to the problem of environmental influences is two-fold: the ordinary comparison of identical and fraternal twins (50 pairs of each type) and a study of 19 identical pairs reared apart in environments differing in varying amounts. The diagnosis into fraternal and identical types was much more carefully done than in previous studies of twins (prior to 1927 when the present study began), and resemblances were determined for a large number of traits—physical, mental, educational achievement, and personality and temperament (some psychologists will wonder why the Downey will-temperament tests were included). In addition to the usual comparison of resemblances, Professor Holzinger has provided an interesting statistical technique for evaluating the relative influence of heredity and environment.

The most important and crucial evidence in this study on the influence of environmental factors comes from the data on the 19

* Newman, H. H., Freeman, F. N., and Holzinger, K. J., *Twins: A Study of Heredity and Environment.* Chicago: The University of Chicago Press, 1937. Pp. xvi+369.

pairs of separated identicals. Although the number of cases is unavoidably small, there seems to be some justification for concluding that differences in environment lead to differences in intelligence. One-half the book is devoted to the case studies of the 19 separated pairs. Like most case studies, these are interesting but not productive of generalizations. The student who is interested in the biological aspects of twins will find chapters 2–4 very instructive.

Let us now turn to a critique of certain methods and results in the study. Minor criticisms which might be labeled as mere quibbling will be omitted, and such matters as might be claimed to be the opinion of the reviewer will be designated as opinion. Page references in what follows will be to the book under consideration and as an aid to exposition, the main points will be numbered but not in order of importance.

1. It is tacitly assumed (pp. 28, 31) that because twins were taken as they came (the reader must guess as to the specific method used in locating the twins), unselected samples were obtained. Surely the authors were aware of the fact that the fraternal type twin pairs which are least alike physically and mentally are most apt to be overlooked when twins are being located. Perhaps this type of selection accounts for the resemblance of their fraternal pairs being, in general, greater than .50 for both physical and mental characteristics.

2. The critical reader will no doubt find it difficult to decide whether or not the physical measurements were utilized in diagnosing monozygocity. This is not irrelevant, since sweeping conclusions are later based on the comparison of the physical with the mental resemblances. Regarding this point, we find on page 55 that the provisional diagnosis based upon preliminary inspection was "reviewed in the light of the detailed examination of the palm prints and fingerprints. In this review all the observations *and physical measurements* were taken into account, and the final diagnosis was then made" (italics ours). On page 64 the reader is reminded that "the calculation of the composite physical difference was made long after the diagnosis into fraternal and identical was made. It is therefore rather remarkable that, when the physical measurements came to be tabulated" a "noteworthy" correspondence between the composite physical measurements and the classification was found. If we are to take the statement quoted from page 55 at its face value, there is nothing so remarkable or noteworthy about this finding. From their Table 9, and subsequent discussion (p. 66),

we learn that in 3 cases the tentative or preliminary diagnosis was reversed, but it is impossible to decide just what factors led to these reversals. In searching for further enlightenment we find on page 92 that " the diagnosis based on observation at the time of the examination was practically identical with the final diagnosis based on the entire set of physical measures, being reversed in but 3 out of 100 pairs."

3. The most crucial point, however, concerning methods of diagnosis and subsequent generalizations regarding the hierarchal order of resemblance for the physical, the mental, the educational, and the personality traits is to be found in the fact that the number of finger ridges, one of the several traits for which resemblances are determined, is very definitely related to fingerprints, which played a large part in the diagnosis. The number of finger ridges is partly dependent upon the type of fingerprint pattern. In general the presence of a whorl pattern permits a larger number of ridges than does a loop pattern. If the authors attempt to refute this, they must deny the observable facts in their Table 43 regarding the finger-pattern formulas and number of finger ridges for 38 individuals. Straightforward calculation leads to a correlation of .79 between the number of finger ridges per individual and the proportion of the 10 finger patterns which are of the whorl type. Thus when the authors compare twin resemblances for finger ridges they are unwittingly using a trait which is not experimentally independent of the method of diagnosis. For an analogous situation the authors justifiably criticize Hirsch's selection of similar twins, a procedure which they state " involves reasoning in a circle or begging the question " (p. 10). Hirsch's comparison is said to be " invalidated by the fact that the two groups [were] selected in such a way as to exaggerate the contrast between them " (p. 11). The same thing can be said with regard to comparisons based on finger ridges in the Newman-Freeman-Holzinger study.

Because of the failure to note this " circular reasoning " in their own case, many statements and conclusions made by the authors must be drastically qualified. For instance, in comparing fraternal with identical pair differences in number of finger ridges, a sharp contrast is noted from which it is concluded that " finger ridges evidently constitute a very definite means of determining the proportion of the resemblances between twins which may be attributed to heredity " and " the resemblances of the two types of twins in number of finger ridges, then, may be used as a norm with which

to compare the resemblance in other traits" (pp. 84, 85). Obviously these statements will be deleted as far as scientific acceptance is concerned. A few pages later (89–91) one finds that finger ridges constitute 1 of 5 physical characteristics in a composite difference which is contrasted with a composite based on 4 mental traits. Is not this somewhat invalidated? Incidentally, if one were to have 9 traits, each of which individually showed the same contrast between the 2 groups of twins as regards pair differences, a composite based on any 5 of these traits would show a greater contrast than that based on 4 traits. Have the authors loaded the dice in favor of a greater contrast for the physical over the mental composite when they include 5 traits in the former and 4 in the latter?

When we turn from the contrast of twin differences to the more meaningful comparison of resemblance coefficients, it will be noted that when the spurious values for finger ridges are eliminated from Tables 25 and 26, it does not follow that "the general trend appears to be for greatest difference [between identical and fraternal] in correlation for physical traits; next, mental tests; . . ." (p. 99). Also the conclusion drawn from the h-square or variance technique must be modified. As might be expected, h^2 is largest for finger ridges. Disregard these values and it is then impossible to claim that h^2 is larger for physical than for mental (intelligence) traits, and therefore by this so-called more rigid method it does *not* appear "that the physical characteristics are least affected by the environment, that intelligence is affected more; . . ." (p. 353). We are not arguing that this statement is not true; we are merely saying that it is not supported by the data unless one accepts the exceedingly doubtful results for finger ridges.

4. We next raise a point regarding mathematical accuracy *versus* common sense. In order to determine the significance of the difference between identical and fraternal resemblance coefficients, the raw r's were transformed to z's *and* a corrective factor somewhat smaller than .009 was uniformly added to all the z's (p. 96). Obviously this legitimate refinement will not alter the difference between z's, whereas the use of the more sensible partial r's (age out), which likewise may be transformed to z's with a loss of one degree of freedom so far as sampling is concerned, will lead to increased differences between z's for identicals and fraternals which in some instances amounts to as much as .20. Since the standard error of the difference between z's derived from partials is .205 for the

data at hand, it is seen that a relatively large correction (for age) was ignored at a time when a minute mathematical correction was employed. For example, the use of partial instead of raw correlations raises the critical ratio from 3.1 to 4.4 for height and from 2.0 to 3.0 for Binet mental age.

5. The authors conclude from their data that fraternal twins "grow less alike" (p. 353) in mental traits (M.A. and E.A.) with age, and therefore that such traits are modifiable by environment. This conclusion is reached after making the following comparisons for younger *versus* older twins:

a. mean pair differences for younger and older

b. these differences expressed as per cent of trait standard deviation

c. raw correlations for younger and older pairs

d. partial correlations (age out)

e. standard errors of estimate

f. regression coefficients for differences on age.

Let us examine these methods and the deductions therefrom. The authors admit that (a) is vitiated because of increasing trait variability with age, whence method (b) is used, but the results obtained by method (b) were not evaluated in terms of sampling. Despite these obvious shortcomings, it is stated that "the difference is much larger in the older than in the younger group, whether we measure by absolute score difference or in units of standard deviation" (p. 108). Surely so able a statistician as Professor Holzinger could deduce that approximately one-third to one-half the absolute differences for both M.A. and E.A. is attributable to the artifactual differences in trait variability, and thereby properly qualify the "significantly larger" idea near the bottom of page 107. (Later we shall question the validity of comparing mean pair differences via the probable error technique). When the results of method (b) are considered in terms of sampling, it will be found that the D/σ_D is about 1.2 for M.A. and about 2.0 for E.A. differences.

Obviously method (e) is subject to the same limitation as (a) so far as changing trait variability is concerned, but this fact is apparently ignored when the authors conclude that the standard errors of estimate "further substantiate the conclusion already reached that the older fraternal twins are less alike in mental and educational age than the younger twins" (p. 109). As to method (c), the reviewer is of the opinion that no scientific importance can be

attached to raw correlations which are so markedly affected by age. This brings us to method (d) in which resemblance coefficients, with age partialed out, are compared for younger and older groups. We raise no objection to this method, but the difference for younger and older fraternal resemblances, via Fisher's z-transformation, is only 1.2 times its standard error for M.A. and 2.0 for E.A. Can it be said from these figures "that the older twins are considerably less alike in these two traits" (p. 109)?

Method (e), which involves the regression of twin differences on age, is highly questionable as utilized by the authors. The regression coefficients for M.A. differences on age are .54 and 1.38 respectively for identicals and fraternals, and for E.A. the values are .41 and 1.15. From these it is concluded that "there is a rather pronounced tendency to become more different [with age] in such traits as mental and educational age" (p. 110). It is said that a plot of the regression "equations brings out the relationship with age very vividly" (p. 110). The joker here is that the authors have failed to appreciate the fact that there will be a marked increase in twin differences with age for these two traits due to increasing trait variability even though the real resemblance (or difference) remains constant. This is obvious from the fact that even though the older identicals are slightly more alike in both M.A. and E.A. as judged by resemblance coefficients (age partialed out), they seem to become less alike as judged by the regression of differences on age. Actually, if we had twins of differing ages but yielding the same resemblance coefficient, r, for each age, the regression of M.A. twin differences on age will be $2i\sqrt{1-r}/\sqrt{\pi}$ where i equals the yearly increment in M.A. standard deviation. Assuming the S.D. for IQ's to be 15, we have $2.03\sqrt{1-r}$. (The assumption of a linear relationship between M.A. trait variability and age will not hold strictly for the data at hand, but it is implied by the authors to be true when they report the relationship between twin differences in mental age and age in a linear form.) Thus if identicals of varying ages show the *same* resemblance, say .86, as reported by the authors for their 50 pairs for M.A., the regression coefficient for differences on age would be .76. Their observed value of .54, when compared with this expected value, is therefore consistent with the slightly higher resemblance coefficient for the older pairs. The regression coefficient of 1.38 for fraternal twin differences in M.A. on age, must be compared with 1.28, which would result if the resemblance of fraternal twins in M.A. were .60 and did not

change at all with age. The conclusion from the regression coefficients that "the twins of a pair become less alike in mental ability as they grow older, particularly in the case of fraternal twins" (p. 110) is decidedly unwarranted as far as M.A. is concerned. It can readily be shown, or seen, that their conclusion from regression equations for E.A. differences on age is also in need of drastic modification because of the changing trait variability with age.

It is the opinion of the reviewer that the authors were too intent on thinking that fraternal twins become less alike with age, that mental differences "do increase to a marked degree" (p. 110), and therefore that these traits (M.A. and E.A.) are modifiable by environment. There seems to be no other way of explaining such rash generalizations, which ultimately must rest on differences which are only 1.2 and 2.0 times their standard errors. Furthermore, the reviewer does not believe that the consistency of a multiplicity of statistical techniques applied to the same data can be taken as corroborative of anything more than the mere consistency of the techniques. A difference (for M.A.) which is 1.2 times its standard error along with a difference (for E.A.) which is twice its sigma is interpreted as "significantly larger," "much larger," "considerably less," "marked," "less alike as found here." Surely a difference which would arise one out of four times where no real difference exists, and which is definitely not in accord with previous findings, is a flimsy basis for such positive statements.

6. We next turn to the treatment of the resemblance data via the h^2 technique, which gives the per cent of twin difference variance attributable to nature providing the following assumptions are tenable:

a. the trait variance for fraternals equals that for identicals

b. nurture influences are the same for both types of twins

c. differences due to nature are uncorrelated with differences due to nurture

d. the traits are not affected by the asymmetry mechanism

e. the variance due to errors of measurement is negligible.

It is stated that an obtained h^2 will be too low because of factors (a), (d) and (e), too high because of (b) and (c), and that the net effect of (a) and (b) can be considered as negligible. The net influence of (c) and (d) is admittedly speculative, and it is said that (e) "may be taken into account by the use of the corrections for unreliability of the test" (p. 121), but only a feeble attempt was made to do so.

It is easy to show on the basis of any reasonable assumption regarding the reliability coefficients that correction for (e) will yield h^2 values of the same order of magnitude for mental (M.A., E.A., Binet and Otis IQ) as for physical traits such as height, weight, and head measurements (omitting, of course, the highly questionable finger ridge data). A word should be said as to what the reviewer means by reasonable reliability coefficients. It is easy to discard the value of .98 for Binet mental age proposed and used by the authors for age constant data. They state that this is "a value found in several other studies" (p. 121). Was age constant? Evidently at this point they forgot the previously (p. 77) made statement that the commonly found reliability is in the neighborhood of .85 or .90. For such trait variance as found in this study, it would be nearer the truth to assume the reliability of the Binet to be .93 or .94. The fact that the Otis score resemblance for identicals exceeds unity when corrected for attenuation by using an odd-even reliability coefficient does not suggest to the reviewer, as it does to the authors, that "these correction formulas . . . overestimate the corrected coefficients for high correlations" (p. 118), but rather that the assumptions underlying the correction formula have not been met. An assumption of .96 as the single age reliability for Otis score will boost h^2 from .77 to .85, and a lower reliability will raise it still more. Assuming .94 as the reliability of the Stanford Binet raises h^2 from .65 to .80. In general, corrections for errors of measurement will raise h^2 about .10 in the case of mental traits and only about .01 in the case of physical traits.

In their summary chapter the authors state that "there is one feature of the results which is not affected seriously by either the uncertainty of the assumptions [underlying h^2] or the limitation of the circumstances under which the correlations are found. This is the relative share of the genetic and the environmental factors in the different classes of traits" (p. 353). It is the opinion of the reviewer that the authors have not only presented no evidence in support of this contention but have also forgotten their earlier admission (p. 115 and p. 121) that factors (b), (c) and (d) probably act differentially with regard to the different classes of traits. Certainly the reliability factor, (e), operates differentially (as indicated above). In the presence of so much ignorance concerning the net effects of the factors influencing the variance technique, it is indeed surprising to find the authors concluding that "by this more rigid method, as by the simple comparison of differences between the

correlations for the two types of twins, it appears that the physical characteristics are least affected by environment, that intelligence is affected more, . . . This finding is significant . . ." (pp. 353–354). This conclusion is all the more startling when the given h^2 values for the 4 intelligence measures, ranging from .65 to .80, and the values for the 6 physical traits (again omitting the nearly meaningless finger ridge data), ranging from .75 to .81, are compared in the light of probable errors of order .05 to .08. The largest single difference—for height and Binet M.A.—is less than twice its probable error, and 2 of the intelligence values exceed 3 of the figures for physical traits. The support given the above quoted conclusion by the simple comparison of resemblance coefficients for the 2 types of twins for physical and intelligence traits vanishes when partial correlations are compared. We have discussed this point under (4), but it should be noted here that 2 of the 4 intelligence measures show critical ratios which are larger than 4 of the 6 ratios for physical traits.

7. The very interesting correlations between trait differences for the 19 pairs of identicals reared apart and rated intra-pair differences in environment would carry much more weight had not 2 of the 5 raters been familiar with the cases and measurements. Furthermore, the statistical significance of some of these correlations may be doubted since only 2 of them exceed 3 times their standard errors. If the authors are willing to place confidence in correlations which are less than thrice their sampling errors, let them explain the fact that for these 19 cases the correlation between differences in finger ridges and differences for Binet IQ and E.A. are .52 and .63 respectively. These are more than twice their standard errors (via the z-transformation) and the larger is 3 times its error. From these values it might be argued that anything can happen with such a small sample, or that these 19 pairs are not all identicals, or that there is an undeniable relationship between finger ridges and mental ability.

8. The second statistical treatment of the data for separated identicals is the comparison of their mean pair differences with the mean differences for identicals reared together. It is found that the differences between these means for Binet IQ, Otis IQ, and E.A. are 3, 4, and 5 times their respective probable errors, but it is not noted that the use of median differences tends in all cases to cast doubt on the statistical significance of these results. The distributions of pair differences are decidedly J-shaped, so one

would expect medians to be more representative than means. Moreover, so far as the reviewer knows, it has not been demonstrated that the distribution of means of samples drawn from such marked J-shaped distributions is near enough normal to justify the use of probable errors. It therefore seems a bit gratuitous to draw the unqualified conclusion that " the differences are significantly greater, demonstrating the effect of environment on these traits " (p. 356). Incidentally, one wonders about the discrepancies in means as one passes from Table 10 to Table 94 and also why certain of the means given in Table 10 can be checked from the distributions in Tables 11 to 21 while others cannot *unless* one changes his notion as to interval mid-points. An error of .5 is of some consequence when it is being argued that the difference between 8.21 and 5.35 is significant.

9. A most unfortunate error is to be found in connection with Table 95, page 346. This table contains 24 variances which purport to be those for double entry twin differences. Every one is in serious error. As a matter of fact it can be shown by a number of devious routes that the variances given are for absolute or single entry differences. Just how this got by is a mystery. Although the basic data have not been reported in sufficient detail to enable the direct computation of the needed variances, the reviewer has succeeded in calculating and checking them on the basis of 2 different mathematical relationships: (a) the double entry twin difference variance is equal to twice the trait variance multiplied by one minus the correlation coefficient of resemblance, and (b) the double entry variance is equal to the single entry variance plus the square of the mean difference. These 2 schemes give concordant results for 12 of the 24 variances, and for 8 others when the given mean differences are adjusted by .5 (see above), but for the remaining 4 the discrepancies are so great as to make one doubt the accuracy of some of the reported figures. These 4 pertain to weight, head length, and head width for identicals reared together and Binet IQ for fraternals. Computational errors do exist in the study; for instance, the variance of 41.85 reported in Table 95 for Otis IQ differences for separated twins should be 36.84, and that given for E.A. should be 283.79 instead of 277.35.

When Table 95 is revised to contain the double entry variances for twin differences, it is found that all the environmental ratios derived from these variances are somewhat reduced, and that the

computed h^2 values agree, as they should, with those previously determined from the resemblance coefficients and reported in Table 37. Indeed, it was the discrepancies between the h^2 values reported in these 2 tables which led the reviewer to suspect that the variances given in Table 95 were erroneous.

10. Our final point has to do with the scientific meaning of raw correlations *versus* correlations adjusted for age and differences in trait variance. In their last table the authors present resemblance coefficients for identicals, fraternals, and separated identicals for ten "basic" traits, and then draw deductions therefrom without any hint as to the effect of differences in trait variance and the effect of heterogeneity due to age and sex. Evidently the dictum given by Professor Holzinger on page 173 of his statistics text "that it is not safe to compare correlation coefficients of any sort from groups where the range of talent or other conditioning factors such as range in age are very different" can be ignored by the master himself.

These irrelevant factors affect the comparison of resemblances for measures of intelligence more than for other traits, so we herewith give a table from which the authors' deductions from raw correlations can be considered along with possible deductions from correlations adjusted for age and differences in trait variation. These adjustments or corrections have been made by first determining the trait standard deviation for constant age and then using the regular formula (p. 95) for correlation involving the variance of twin differences and the trait variance. The trait variance (partial, age out) for fraternals was arbitrarily chosen as the variance to use in determining the resemblance for all 3 groups of twins. Had we used the variance for identicals, the *relative* magnitude of the identical, fraternal, and separated resemblances would be exactly the same as given by use of the fraternal variance. Should allowance be made for differences in range? Why not? For example, the partial trait variances (age constant) for Binet IQ for identicals, fraternals, and separated are 227.13, 240.25, and 157.22 respectively. The reviewer cannot understand why the authors ignore such large differences in trait variance and their effect upon the comparability of the resemblance coefficients. Certainly the assumption underlying correction for range of ability, *i.e.* equal correlation throughout the range, is in this case as tenable as and no more hazardous than some of the assumptions involved in the h^2 technique.

TABLE I. RAW VS. ADJUSTED RESEMBLANCES

	Raw r's from Table 96			Adjusted for Age and Range		
	Ident.	Frat.	Separated	Ident.	Frat.	Separated
Binet M.A.	.922	.831	.637	.871	.599	.619
Binet IQ	.910	.640	.670	.888	.631	.767
Otis IQ	.922	.621	.727	.919	.618	.796

Let us now turn to Table I and to an examination of some of the conclusions drawn from the raw correlations. On the basis of the correlations which have been adjusted for age and differences in range and which are therefore comparable from group to group, we are forced to question the conclusions that "the correlations for the separated cases are . . . fairly close to the values for fraternal twins and much lower than the corresponding values for unseparated identical twins" and "that variations in the intelligence of the separated twins are of the same order as for unseparated fraternals" (pp. 347–348). As a matter of fact it cannot be claimed that there is a statistically significant difference between the resemblances of separated and unseparated identicals for these measures of intelligence.

11. This book provides another illustration of the disconcerting fact that the summary and concluding chapter of an extensive monograph cannot always be depended upon as a reliable source of information concerning the results of an investigation. Indeed, in this case it requires a large amount of tedious work to discover the necessary, but seldom stated or recognized, qualifications for what the authors accept as established facts. Throughout the book, as well as in the closing chapter, there are many positive statements which must be rejected in toto for the simple and compelling reason that they receive little support from the basic data and justifiable statistical manipulations thereof. It seems unlikely that fellow scientists will share with the authors the belief that their evidence is more crucial than any previously available in the nature-nurture sphere of investigation. The injection of educational achievement and personality traits, though interesting per se, introduces data which have little bearing on the classical problem as to the influence of nurture on tested ability, and therefore the unquestioned finding that achievement and personality are dependent partly upon environmental conditions cannot be accepted as of crucial importance in connection with the main problem. With regard to measures of intelligence it is safe to say that when their data and deductions have

been considered in the light of the many necessary qualifications, which we have noted herein, it appears that the only evidence which approaches decisiveness is that for separated twins, and this rests ultimately upon the fact that *four* pairs reared in really different environments were undoubtedly different in intelligence. This fact can neither be ignored by the naturite nor deemed crucial by the nurturite.

BOOK REVIEWS

MENNINGER, K. A., *Man Against Himself*. New York: Harcourt, Brace and Company, 1938. Pp. xii + 485.

Whether one is a Freudian or not, there is no getting around the fact that Dr. Menninger has written a provocative and thought-compelling book in *Man Against Himself*, in which he analyzes the many aspects of partial or complete self-destruction.

Starting from the well-known Freudian hypothesis of man's "will to die," Dr. Menninger with the competent brush strokes of an artist, paints a picture of man, driven by the "death instinct," a creature of terrific conflicts, accomplishing his own destruction, either frankly through suicide, or in abortive, distorted, or attenuated fashion through alcoholism, invalidism, martyrdom, purposive accidents, and self-mutilation.

To the layman, unacquainted with the blind and mysterious forces that are the well-spring of human conduct, Dr. Menninger's theme will seem both terrible and absurd, but the physician, baffled in his daily practice by the patients who "just won't get better," will find corroboration for his own experiences and deductions.

The chapter on chronic alcoholism is of special interest, it seems to me, in view of the fact that a large percentage of the country now seems to be in the process of committing slow suicide by alcohol.

Alcoholism is a sociological problem that is becoming more complex each day. Before any intelligent effort can be made to reduce its ravages, a clear understanding of the causative factors would seem to be necessary. It would be picayunish to find fault with Dr. Menninger's whole-hearted espousal of psychoanalysis as the only cure for alcoholism. We can forgive him for this, for he has in clear, unmistakable language, stated the problem so that all may understand. It is not that men become drunkards through "heredity" or because they are perverse creatures—it is because they have found in it a temporary escape from an inner conflict.

In describing this problem as psychiatric, primarily, and sociological, secondarily, Dr. Menninger, we hope, has helped to hasten the day when we will abandon our present general attitude which, in his words, is one of neglect:

"the psychology of the man impelled to ruin himself by self-poisoning, in spite of disaster, remorse and resolutions to abandon it, some way or other escaped the consideration of the psychiatrists and was left to the clergy, the social workers, the prohibitionists or the devil."

There is plenty to think about, too, in the material Dr. Menninger has presented regarding neurotic invalidism, polysurgery, and malingering. He has restated principles which are becoming more and more evident in the practice of medicine, and which, when more generally known, will transform the practice of medicine as we know it today. That there can be no division of the human creature into air-tight compartments labelled "body," "mind," and "emotions" is now pretty generally conceded. We are beginning to think of the human being as a unified organism, and to consider him in relation to his environment. Dr. Menninger adds the weight of his authority to this concept, and gives further proof of the devastating effect the human mind can have on the human body, not alone in functional disorders, so-called, but in causing actual organic changes. In this he bears out the conclusions reached by Dr. H. Flanders Dunbar and her associates at the Columbia-Presbyterian Medical Center, after prolonged study of bodily changes brought about by emotional conditions.

Naturally, as enthusiastic a follower of Freud as Dr. Menninger is, he would work out a solution to all these problems on a psychoanalytic basis. While a great many of his readers will not be able to accept the Freudian hypothesis in toto, it should not spoil their enjoyment of this book, for Dr. Menninger has synthesized the various aspects of man's destructive tendencies into a readable and stimulating volume.

It might well be read by the physician or surgeon who has not kept abreast of the times and who still thinks of man as a collection of organs and tissue.

C. Charles Burlingame.

The Neuro-psychiatric Institute of the Hartford Retreat.

Braude, Morris, *The Principles and Practice of Clinical Psychiatry.* Philadelphia: P. Blakiston's Son & Co., Inc., 1937. Pp. x + 382.

The author's purpose in writing the book was "to ease the lot of the student of medicine, of the social sciences, and of jurisprudence—or, of the humanities in its broadest sense—in the difficult field of mental diseases." In the introduction the author specifically

mentions the beginner, which indicates that he intended to write a condensed introductory textbook of psychiatry. The difficulties of this task will be easily understood by anyone who, as a teacher, has been engaged in the introductory training of students of psychiatry. He also will feel that it is indeed impossible to adapt a textbook to the different backgrounds of knowledge of students trained in the various disciplines mentioned above without discussing broadly the fundamental details. Thus, for example, the student of medicine will know what the globus pallidus is, while most students of social science and of jurisprudence will not know anything about the finer structure of the brain. Such fundamental knowledge therefore must be supplied to the student who does not possess it. This necessity again makes it impossible to give a review of psychiatry in such a small book as this without leaving any questions open to the student of one or the other fields.

The author is certainly successful in arranging the material in such a manner that it is "made interesting." He stimulates interest in the present stage of our knowledge by giving a short general historical survey in the beginning, and a brief historical summary as an introduction to most chapters, with occasional reference to belles-lettres. Moreover, the reading of the book is certainly facilitated by the author's reference to hypothetical and real cases.

The introductory chapters give an excellent survey on etiology, pathogenesis, diagnosis, prognosis, treatment, and examination and are obviously written as a result of extensive practical experience. The description of the nature of psychosis, neurosis, and psychoneurosis, is followed by a special section treating with simulation, and malingering, mental deficiency and psychoanalysis. The author, while recommending the classification of mental disease sponsored by the American Psychiatric Association, deviates from it in his text for teaching purposes. This deviation has not been always successful. Thus, for example, Chapter V has the heading: "Primary, Endogenous or Psychogenous, Psychoses," and includes

"Paranoia and Paranoic States. A. Paranoia."
"The Endogenous Psychoses. B. Folie à deux."
"The Reactive Psychoses (Endogenous) C. Prison Psychosis."

These headings would not help the student very much in understanding the interrelation of the various psychoses.

Without going into full details I think that numerous corrections should be made in the book. For example, it would be better to

use the expression "congenital syphilis" instead of "hereditary syphilis" as used by the author. Even for the purpose of simplification Binswanger's presenile psychosis, Alzheimer's disease, Presbyophrenia, and arteriosclerotic dementia should not be given as "synonyms." The most constant changes in Huntington's chorea are described as being located "in the extrapyramidal regions, as, globus pallidus, red nucleus, but also in the cerebral cortex." All of these statements are of little value for a non-medical student, and for the medical student it would have been better to stress the most constant changes in the putamen.

In writing a book specifically for students it is always difficult to anticipate exactly what the student will already know when he reads the book and what the student has to be given to create a reliable bridge between his former knowledge and the new field of science he is now entering. Therefore, the true value of a book like this can be judged definitely only from practical use.

HANS STRAUSS.

N. Y. State Psychiatric Institute and Hospital.

HELWIG, PAUL, *Charakterologie.* Leipzig: B. G. Teubner, 1936. Pp. xii + 295.

As a scientific study in human character Dr. Helwig's book is doubtless an original and important contribution, despite his limiting himself to discuss only the "modern general characterology in Germany." The book is divided in two parts: (1) "general," (2) "descriptive." In the "general" part the author explains etymologically the Greek word's (χασακτής) three-fold meaning—important for his conception of character—and discusses the concept and essence of character as differing from psyche. Viewed from an elevated level, characterology and psychology are two distinct versions of one subject matter—the soul. Characterology's method is based upon four principal features.

(1) Characterology points out the soul's active relation to the outer world. Soul is "Frontstellung" (front-line position) in the struggle with life.

(2) Characterology emphasizes the "Werdeprozess" (formative process) of the soul's structure. Character is the forming energy, the coining power of its own essence; therefore, "steigerbar" (enhanceable), with an infinite expressionability.

(3) Characterology accentuates the "Zentriertheit" (centeredness) of the soul's life, the ego, and deals with personality. In

characterology the soul forms its own personality, and character is juxtaposed to personality [1] (p. 12); whereas in psychology the soul is a democracy of different faculties.

(4) Characterology stresses "Wertmoment," *i.e.* the significance of the evaluation for the "ego," because (a) evaluation is a constituent of character; (b) a person's expressions reflect upon and help form his character; (c) sympathy and antipathy, the pillars of evaluation, are important characterological factors in our general recognition. The author seems to endorse L. Klages, the most extant figure in the struggle for characterology, who, in his passionate partiality for "life" against "spirit"—disconnected from life—distinctly propounds the significance of evaluation for the approach to character. Helwig concludes his exposition of the characterological method by shedding new light on the peculiar essence of the soul and the entire physico-psychic problem. He maintains that his description connects the soul much closer to the body in spite of their radical differences, by virtue of the soul's auto-creative energy.

The nearest and most important link in the long chain of the characterological process is the individuum's own body. The "Ausdruckslehre" (doctrine of expressions) and its scientific field (symbolic) finds a particular treatment (pp. 23, 272). Graphology (pp. 283–286), one of its dynamic branches, claims today, particularly by reason of the exhaustive work of L. Klages, to be an individual science.

The second part may well serve as a compendium of the many important attempts made to solve characterology's fundamental problem; namely, to recognize the individual character through the "common to all." It would be rather futile for the reviewer to summarize the wealth of material that is not only accumulated but critically sifted and presented in a form that would not permit any adequate abbreviation. Suffice it to say that the problematic essence of the "common to all" is responsible for the various conceptions in the characterological methodology.

(1) Everyday characterology seeks the "common to all" in so-called characteristics (Charaktereigenschaften): this method, which catalogues only common character qualities, is prescientific and thus to be disregarded (pp. 32–35).

(2) The methodical organization of the "common to all" accord-

[1] Ludwig Klages, *Die Grundlagen der Charakterkunde*. Leipzig, 1928; and Paul Häberlin, *Der Charakter*. Basel, 1925.

ing to types, referred to as "Typensysteme (typologies) and subdivided into (a) with systematic structure, (b) without systematic structure, (c) medicinal typologies (Kretschmer and Jaensch), (d) race types, (e) sex or erotic types (pp. 35–163).

(3) A third form of the "common to all" is presented through the so-called "basic components" of character—urges, instincts, wishes, etc.—the dynamic functional factors of character; called "Aufbausysteme" (constructive systems; pp. 164–211).

(4) A fourth form originates from the derivation of the characterological—"common to all"—from factors that do not pertain to character, referred to as "Erklärendes Systeme" (explanatory systems—pp. 212–226). The purest form of "Erklärendes Systeme" would show the derivation from physical factors.

The crucial question: "From which viewpoint is an organization of character possible" leads Helwig to a thorough examination of the concepts of "Charaktereigenschaft" and "type." Types, fundamentally different from the general concepts (limited and defined through comprehension and extension by traditional logic), signify purely infinite directions of expressions; the determination of their essence and definition will offer modern logic a new field of speculation.

After a discussion of the philosophical point of view on the character problem, taking Paul Häberlin as a representative (pp. 266–272), Helwig asserts that no single system is able to give a satisfactory picture of a man's character, for the number of perspectives under which it can be seen is infinite. The author is well aware of the limited positive results which characterology offers for practicable purposes—education and all social phases; since the understanding of the individual through the "common to all," in the end, must be unsatisfactory. The reason is not that the instruments of science are insufficient, but that character in itself is a mystery. The final experience of its irrationality is rather the subject of religion; not of knowledge or recognition, but of the immediate intuition of faith (pp. 291–295).

Undoubtedly Helwig knew how to profit from antecedent investigations such as the dynamic (Gestalt) and, particularly, individual psychology. His definition of character "as a process of growing coinage, of increasing formation and structure, and thus of expression on the outer world" (pp. 260, 7) is sharply contrasted with that of associationism, or of any system which considers the essence of character as "the organization of all so-called mental contents,

traits, capacities, and reaction tendencies." It is evident that the concept of character as defined by Helwig has no place in psychoanalysis; character would commence where analysis ceased to be, *i.e.* beyond the "restricted system" of the household in which all-powerful libido is the only force (pp. 212–230). A soul forced as a mechanistic model [2] can never be or become a character.

Helwig considers it a great step for modern propounders of individual psychology (f.i. Fritz Künkel,[3] pp. 242–249) to have liberated the *healthy* soul out of Adler's (still "restricted") pessimistic mechanism (p. 238) and to have underlined the dynamic in character; in propagating the ethical aspect they have set up a "Sollensforderung," *i.e.* a vigorous appeal to the soul's ability and obligation. Individual psychology of today is the proud "open system" which exhibits all objective powers of the soul in the most neutral sense of the word. Character now is not conceived of in the category of being, but as a potential unit of ability, volition, and obligation. This "Sollensforderung" is not directed by a positive *de*scription but by a negative *circum*scription of character; for individual psychology shows in well arranged type-dimensions what a healthy character should not be, thus bringing forth the most original and fruitful feature of its very ramified doctrine. The author, however, does not fully endorse individual psychology; he criticizes it for regarding soul free of all "Leitlinien" (determining factors)—physical and psychical—as the ideal, and thus leaving the problem: "how can soul individuate itself without hindrance?" unsolved; only in hindrance lie the roots of freedom. Helwig offers his own solution (pp. 249–251) within the limits of individual psychology; he corrects its hypothesis "freedom of any hindrance" by introducing as presupposition "an elastic tension between freedom and hindrance." Without doubt Helwig has also utilized in this book thoughts of his recently published treatise on the physico-psychic problem—"Seele als Äusserung" (soul as expression).

Helwig's book manifests a skillful handling of his thesis, and a competent familiarity with the literature that is affixed to each chapter; regrettably it lacks indices of names and subjects. Save for some arbitrary interpretations, a few of which may invite criticism (*cf.* the mentioned "openness" of Kant's categoric imperative, p. 13,

[2] L. Berman, *The Glands Regulating Personality*. New York, 1922; and J. B. Watson, *Behaviorism*. New York, 1924.

[3] Fritz Künkel, *Die Arbeit am Charakter* (Nineteenth Ed.). Schwerin, 1934.

and the rather bold conception of Klages' critique, p. 193, etc.), the author's thoughts, proceeding upon assumed fundamentals, are set forth with clear logic and unbiased forwardness.

His vigorous, though not always fluent, style suggests a sincere probing soul and an " open " character, which even the reader who may disagree in detail with Helwig's thoughts must recognize. The terminology is largely his own, and at times envelops the profundity of his thoughts. Thus, to gain the full benefit of this important work, one must read it carefully, then reread it. The systematic presentation of the fundamentals in the " general " part, and the succinct yet clear description of the various phases brought to light by modern psychology, psychiatry, and philosophy in the second part, enriched by the author's " sine ira et studio "-critique, deserve the close attention of all who are interested in the problem.

LUDWIG SCHOPP.

New York.

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NOTES AND NEWS

Dr. John Edgar Coover, emeritus professor of psychology at Stanford University, died on February 19 at the age of sixty-five years.

—*Science*.

Leonard Carmichael, now professor of psychology and dean of the Faculty of Arts and Science at the University of Rochester, has accepted the presidency of Tufts College. The trustees of Tufts College have simultaneously established a laboratory for research in sensory psychology and physiology which will be under the direction of the new president. Mr. Bertram Wellman, now research associate in psychology at the University of Rochester, an expert in the application of electrical techniques to psychological and physiological problems, will become assistant director of the laboratory.

Dr. Lyle H. Lanier, assistant professor of psychology at Vanderbilt University, has accepted an appointment as professor of psychology at Vassar College. Dr. Lanier succeeds Dr. Margaret Floy Washburn, who became professor emeritus in June, 1937.

Dr. J. E. W. Wallin, director of special education and mental hygiene for the State of Delaware and lecturer this year in the extramural department of Rutgers University, will return to Duke University in the summer sessions of 1938, offering graduate courses in mental hygiene and mental and educational retardation. During the year Dr. Wallin has served on two commissions in Delaware, namely, as secretary of the Constitution Sesquicentennial Commission of Delaware and member of the Delaware Swedish Tercentenary Commission under gubernatorial appointment.

The Fourth Annual International Congress for the Unity of Science will be held at Girton College, Cambridge, England, July 14 to 19, 1938. The chief topic will be the unification of scientific language. A special session will be devoted to fundamental problems of objective psychology and to psychological terminology. The Congress is organized by the International Institute for the Unity of Science, 267 Obrechtstraat, The Hague, Holland.

THE Psychological Institute of the University of Vienna will offer the following courses for English-speaking students in its seventh annual summer school in psychology, July 11 to August 6, 1938: *Fundamental Principles of Psychology* (Karl Bühler); *Principles of Developmental Psychology* (Charlotte Bühler, Lotte Schenk); *Experimental Psychology* (Hans Herma); *Applied Psychology* (Käthe Wolf); *Viennese Tests for Children* (Liselotte Frankl, Edeltrud Baar, Rudolf Gröger); *Diagnosis and Treatment of Problem Children* (Charlotte Bühler, Lotte Schenk); and *Human Personality* (Peter Hofstätter). Further information regarding the summer school and living arrangements in Vienna may be obtained from Dr. Henry Beaumont, Department of Psychology, University of Kentucky, Lexington, Kentucky.

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